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Democratic Politics and the Culture of American Education

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This article argues that weaknesses in the school's socialization of democratic values can be traced to culturally patterned strains in American education. Such strains are cultural adaptations to a conflict between educational knowledge and order, on the one hand, and egalitarian politics in America, on the other. After treating a defective explanation for the school's weakness as a democratic socialization agent—the "hidden curriculum" approach—the article outlines the conflict between democratic politics and "the basic shape of schooling." The article concludes by tracing the deleterious effect of this conflict on teachers, curricula, and students.

In 1916 John Dewey argued that a democracy should pursue "a type of education which gives individuals a personal interest in social relationships and control, and the habits of mind which secure social changes without introducing disorder" (Dewey, 1916, p. 115). As if in reply to Dewey, consider William Blake: "But to go to school on a summer morn? O it drives all joys away/ Under a cruel eye outworn/ The little ones spend their day/ In sighing and dismay" ("The Schoolboy"). Blake's description of schooling could hardly be farther from Dewey's recommendation. Although Blake wrote his lines 200 years ago, I stumbled across them in reading Phillip Jackson's frontispiece to his influential *Life in Classrooms* (1968); Jackson apparently believes that time has not diminished the truth of Blake's observation. The question remains: how can education strengthen the cooperative social conscience that lies at the heart of Dewey's vision of democracy when the beneficiaries of education consider themselves its victims, involuntarily bound in the harness of schooling? Can schoolchildren learn freedom and equality when schools enforce constraint, hierarchy, and inequality? It is this question—broadly construed—that I address here.

Though Blake might have been the last to take comfort from empirical social science, much of what we now know about "democratic political socialization" accords with his insight. Simply put, schooling has yet to demonstrate its contribution to the child's development of democratic values (Dawson, Prewitt, and Dawson, 1977, pp. 143–44). Pertinent research illustrates the point. Jennings', Ehman's, and Niemi's examination of the social studies classroom concludes that variations among teachers have little impact on democratic political attitudes among students (1974, Ch. 8). If teaching styles seem ineffective, what about curricula? The most comprehensive examination of curricular impact is that of Jennings and Niemi, who write, "Our findings certainly do not support the thinking of those who look to the civics curriculum in American high schools as a major source of political socialization. When we investigated the student sample as a whole we found not one single case out of the ten examined in which the civics curriculum was significantly associated with students' political orientation" (1974, p. 205; see also Mercer, 1973). Finally, what about the effects of curricula and students jointly considered? Perhaps we have been wrong to focus mainly upon individual teachers or individual students rather than upon whole schools. After all, from the student's point of view schooling is a synthesis of many experiences, not a collection of isolated encounters with particular teachers or subjects. But even this alternative does not avail. Jennings writes that when schools themselves become the units of analysis, "classroom and curriculum leave but marginal residues on the political character of high school students" (Jennings, 1974, p. 406).

Of course, as Willis Hawley points out (1977, pp. 319–25), much relevant data remain outstanding. It is hard to believe the schools are not having *some* impact on democratic values, even though research stubbornly refuses to identify what that impact is. Nor do Americans rely solely on schools for their democratic socialization; perhaps we become "democratic" despite the school's efforts. Nevertheless, there appear to be a few successes in the school's teaching of democratic values. For example, marked interest in politics emerges in schools composed mainly of college-found students—

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even among less promising students. It is unclear whether to attribute these effects to a "trickle-down" process—with college-bound students picking up participatory ideas from their teachers and passing them along to other students—or to a "bubble-up" process—with teachers fitting their instruction to the advanced capacities of their better students, and thus unintentionally benefiting poor students as well (Langton, 1969, Chs. 4–5). Either way, however, these findings solve nothing, for they indicate only that the inefficacy of schools in teaching democratic values may proceed as much from characteristics of the student body as from the school itself. The point remains, therefore, that as a planned enterprise in the teaching of democratic values the American public school's share in the general process of democratic socialization appears at best suspiciously problematic (Shaver, 1977, pp. 300–07).

This problem must be viewed alongside another debate currently raging in educational circles: the debate about the relationship between schooling and occupational achievement in America. On the latter subject, too, Deweyan "naive optimism" has given way (see Ravitch, 1978). Of course, we may lack our own Coleman Reports, our Christopher Jenckses and our Michael Katzes, but our efforts should be seen in conjunction with theirs. In fact, though conducted entirely independently, the two research efforts—one into the effects of education on democratic socialization, the other into the effects of education on student achievement and economic success—reach the same conclusion: namely, that the American school leaves much to be desired. The two lines of research combine to make a powerful criticism of schools, for what Averch et al. say about schooling in general applies as well to schooling and democratic values:

> Research has not identified a variant of the existent system that is consistently related to student outcomes.... *Research has found nothing that consistently and unambiguously makes a difference in student outcomes* (1972, p. x, emphasis in original).

But ours is not just an educational problem, for education is a major arena of public policy. Educational failures are, ipso facto, policy failures. Indeed, more even than policy is at stake. Whether or not their faith was warranted, generations of political theorists and politicians have looked to education to produce a democratic citizenry. Although Thomas Jefferson and Lawrence Kohlberg would disagree about many things, they would surely agree on one point: that only such a citizenry can protect itself from power-hungry leaders and thus stabilize democratic institutions. Without an informed, participating public, the credentials—and the futures—of systems calling themselves democratic must be considered suspect. The failures of democratic education are therefore failures of American politics—as well as puzzles for democratic theory (Dahl, 1956).

In this article I reconsider the character of democratic education in American schools. I first argue that the dominant contemporary interpretation of failures in American democratic education—the theory of the hidden curriculum—is incomplete. I modify this theory by highlighting the conflict between a primarily authoritarian context and the democratic content common in American political education. I then employ my modification to explain salient organizational, cultural, and educational features of American schooling—and to show why such schooling is so peculiarly self-defeating and ineffective.

The Hidden Curriculum

The most widely-held contemporary explanation for the weakness of democratic schooling is the "hidden curriculum," a set of common practices which, by teaching quite different behavior and power relationships, supposedly prevents the transmission of democratic values in the school (Apple and King, 1977, pp. 108–27). It is alleged that the school teaches hierarchy, not democracy. Instead of student power in the school we find teacher control over curriculum and administrator control over the school building. Instead of genuine equality among students we find invidious ability groupings (Keddie, 1971, pp. 133–61). Instead of liberty for students we encounter constant surveillance. Instead of the "personal interest in social relationships" envisaged by Dewey, we observe egoistic competition for grades, for status, and, ultimately, for admission to "appropriate" colleges and universities. Instead of the democratic citizen's enjoyment of choice and spontaneity, we discover the dead hand of delay and queing (Jackson, 1968, Ch. 1), of teacher dictates, of a fixed, externally prescribed, stultifying curriculum. Behind the pretence of "democratic" socialization lurks the reality of closely supervised, standardized training where students fight each other in order to please those in power. In sum, students cannot learn democracy in the school because the school is not a democratic place (Cohen and Lazerson, 1977, pp. 133–61).

Though appealing in many ways, the hidden curriculum argument does not provide a comprehensive explanation for the school's failure to teach democratic values. It cannot provide such an explanation because it displays internal flaws which demand correction. These problems embrace logic, description, conceptualization, and inference.

Logic. Let us begin with the problem of logic. Suppose we find the majority of students in a school apparently submitting to the school's hidden curriculum–identifying with their teachers, competing with each other for grades, conforming to authority. The hidden curriculum is working, and therefore democratic socialization cannot take place. Fair enough. But now suppose the opposite: that students hate their teachers, resent their school, and actively protest against the hidden curriculum. These facts indicate the presence of alienation, and alienation hinders schooling. Thus, both submission and resistance among students demonstrates the hidden curriculum to have prevented democratic socialization in the school (Young, 1971).

The logical flaw in this position fairly jumps out at the reader. The hidden curriculum argument becomes non-falsifiable if neither acceptance nor rejection of schooling can dispute it. Non-falsifiable positions may be true, of course, but it is impossible to believe them so without the intervention of faith. And the requirement of faith is not a virtue in most social science quarters.

Description. There are also problems of description. Simply put, description of the hidden curriculum as uniformly repressive is wrong. Any reader of Philip Cusick's *Inside High School,* an ethnographic account of a middle-class American high school, might be impressed by the amount of freedom students carve out for themselves in school. As Cusick puts it, "The school . . . provides an enormous amount of time when students are actually required to do little other than be in attendance and minimally compliant. It is this that provides [the students] with the time to carry on their group activity, and their group activity seems to consume over half the school day" (1973, p. 214). Indeed, if teachers attempt to impose an authoritarian hidden curriculum on students, students return the favor by attempting to impose a lenient hidden curriculum of their own on teachers (Cusick, 1973; see also Swift, 1971, pp. 31–65).

But this observation is itself misleading, for it assumes that respect for authority is uppermost in teachers' minds and that failures in this regard are due solely to student resistance. Teachers cannot ignore problems of authority, of course, but an equally important aim for many teachers, as Dreeben points out (1977, pp. 544–49), is to develop students who are active, purposeful, forceful, and assertive in their pursuit of academic goals. This component of teaching clearly conflicts with the hidden curriculum's supposed encouragement of submissiveness and passivity.

Moreover, in recent years authoritarian aspects of the hidden curriculum have come under legal attack. Recent court decisions establish important areas of student rights regarding political action within the school, grooming, and student publications (Fischer, 1977, pp. 249–63). Today's students are almost certainly less victimized by an authoritarian hidden curriculum than were their predecessors. How can such a divided hidden curriculum be held responsible for the school's failure to socialize democratic values effectively?

Finally, many descriptions of the hidden curriculum are insufficiently sensitive to problems of comparison. For example, how does the authoritarianism of the hidden curriculum fit within the culture from which students are drawn? It is possible that the hidden curriculum is actually less authoritarian than the students' cultural background, in which case one can hardly hold the hidden curriculum responsible for the absence of democratic attitudes among students. Take an example. Some writers draw an implicit contrast between what they take to be the authoritarianism of the school and what they take to be the democracy of working-class culture, a comparison which leads them to conclude that the hidden curriculum effectively turns innocent working-class democrats into cynical authoritarians. A reading of Paul Willis' *Learning to Labour* (1977) is a useful corrective for such romanticism. Racism, sexism, and glorification of violence permeate the culture of Willis' English working-class boys. In comparison with these values, the awkward autocracy of the hidden curriculum becomes, if anything, a modest step toward democracy (Glock et al., 1975, pp. 61–103). The point is simple: antidemocratic sentiments can be found in all cultures and all social classes. Therefore, the presence of antidemocratic attitudes among students does not demonstrate the impact of the hidden curriculum, even if such a curriculum *is* authoritarian.

A related mistake is also common: some hidden curriculum theorists exaggerate the difference between the messages the hidden curriculum conveys and the messages contained in student culture. As Harold Entwhistle argues, the culture of working-class students and the culture of a middle-class school are not as opposed as hidden curriculum theorists claim. Working-class people generally favor order and discipline, a view shared by teachers from working-class origins who often become prime exponents of an authoritarian hidden curriculum. As Entwhistle points out, "*If* teachers with working-class origins typically resent lower-class students and despise their cultural values, the explanation may lie in phenomena related to working-class stratification itself, not in the middle-class values" (1978, p. 48).

Inference. The third problem in the hidden curriculum argument is inferential. Hidden curriculum theorists infer that because the school renders students dependent, students accept their dependency. Much evidence disputes such a view, even for those students who have most to gain by remaining dependent. Consider the fact, for example, that it was academically successful students who during the 1960s led rebellions against the school (Keniston, 1973). Is this the picture of a hidden curriculum effectively creating a passive, dependent—albeit advantaged—clientele? I think not.

Of course, it may be that academically successful students retain the personal resources necessary to challenge the hidden curriculum, and that dependency and conformity are clustered among "slow" learners, whose egos may have been irreparably damaged by failure in the scholastic competition that is so conspicuously part of the hidden curriculum. But this is not so. In his study of academically backward English working-class students in a boy's school, Willis shows that "the lads" effectively resist the hidden curriculum. These students are not dependent, they do not feel themselves to be failures, and they do not envy schoolmates who make good grades. Instead, they despise the servility of students who do well in school, and they believe their own academic failure to be a sign of worldly success, precocity, and masculinity. They channel their contempt into active opposition to the school by exploiting every loophole the school's cumbersome bureaucratic structure affords. Either they evade the school's rules and regulations entirely or they distort disciplinary procedures so as to disrupt the educational process. The hidden curriculum serves only to reinforce their anti-scholastic values, not to convert them to passivity or dependence.

Nor is regularized opposition to the hidden curriculum a purely working-class phenomenon. In his path-breaking *Adolescent Society*, James Coleman dwelled upon such phenomena as the "dating and rating" game, the stress upon appearance and, for boys, athletics, the sacrifice of scholastic performance for social popularity, the appeal of fraternities and sororities—all middle-class American counterparts of the violence, the racism, the sexual exploitation, and the pseudo-masculinity of Willis' British working-class students (Coleman, 1961, Chs. 2–5, 10). Students, whatever their class or nationality, find ways of protecting themselves from curricula.

Why these problems of inference from the presence of a practice to its supposed effect? One reason is that hidden curriculum theorists resemble those who write about democratic socialization in their devotion to what Ted Tapper has termed a "monolithic" conception of socialization (Tapper, 1976, p. 15), in which socialization agents carefully coordinate their activities. Were socialization monolithic in this sense, merely to identify a schooling practice might be enough to demonstrate its effectiveness. But the assumption itself seems to oversimplify a quite complex reality.

Equally important, however, is the fact that, from the student's viewpoint, the "hidden curriculum" is not really hidden at all. As Michael Apple and Nancy King point out, public schools in America have always emphasized their intention to teach discipline, obedience, and conformity. Teachers still explain at length (and with heat) to students the benefits supposedly to be derived from acceptance of the hidden curriculum: the virtues of promptness, neatness, and respect for authority, for example. Such exhortations not only sensitize students to the presence of a "hidden curriculum," but also suggest, unintentionally of course, that sloth, laziness, and defiance can subvert the curriculum (Apple and King, 1977). A hidden curriculum so unsubtle can hardly be expected to succeed.[1]

Conceptualization. The fourth and final difficulty with the hidden curriculum argument is its faulty conception of the relationship between social class and schooling practices. Some writers claim that, the 1960s notwithstanding, we should expect to find more conformity to the hidden curriculum among socially advantaged students than among disadvantaged students. After all, conformity among the former insures the inheritance of a favorable class position. By contrast, disadvantaged students have little prospect of occupational success, and therefore no incentive to accept schooling, including the hidden curriculum (Stinchcombe, 1964, Chs. 1 and 2). These expectations are in fact self-fulfilling. Rejection of the school by disadvantaged students seals their dismal fate by denying them needed educational credentials. Acceptance of the school by middle-class students insures their future success. In this way contemporary capitalism reproduces class inequalities from generation to generation (Ravitch, 1978, pp. 75–100). Thus the "requirements" of social class position explain what might otherwise be puzzling about the hidden curriculum's effectiveness.

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[1] In a sense, therefore, hidden curriculum theorists actually refer to a dual curriculum, one involving subject matter, the other authority relations. Neither is really hidden. However, in keeping with literature and theory on the subject, I will continue to refer to the hidden curriculum when I describe authority relations in teaching.

There are several flaws in this argument. For one thing, the hidden curriculum that confronts middle-class students is not markedly authoritarian. In fact, as Edward Morgan points out, "honors track students" (who are mainly middle-class) "have the most *democratic* learning experiences and general track students the most profoundly undemocratic experience" (1977, p. 96, emphasis added). Why a hidden curriculum purportedly intended to train middle-class students to take positions of authority in hierarchical institutions should present a democratic face to these future leaders can only be considered a mystery. True, these lucky students will be *running* institutions and can thus enjoy comparative freedom. But from a sociological point of view, habitual respect for and acceptance of authority is just as important a quality for those who run institutions as for those who are run *by* institutions. Any lapse in respect for authority among leaders may reverberate downward, emboldening subordinates to question their own subordination. After all, followers often imitate leaders. In sum, the argument that among leaders democracy is "affordable," even necessary, is simply not convincing.

An additional problem with this argument is its interpretation of the connection between social class and the hidden curriculum. Not even the most committed hidden curriculum theorist claims that the school purposely searches out middle-class students for the kid-glove treatment practiced in honors tracks (but see Bowles and Gintis, 1976, pp. 125–41). In any case, there are enough working-class students in honors tracks to belie any such argument. Therefore, it must be educational content, not the social class of students per se, which explains the hidden curriculum's differential treatment of students. Honors students receive democratic treatment not because they are middle class, but because they are in honors tracks, where the subject matter permits somewhat more creativity, independent judgment, personally structured investigation, and informed debate. Of course, teaching does have consequences for class structure, but these consequences are realized only through teaching itself. Therefore, variations in the hidden curriculum must be understood in terms of the internal dynamics of educational systems as well as the external dynamics of social class.

A last problem of inference is the assumption that students apply what they learn in school to politics. This is another mistake critics of the school share with theorists concerned with democratic socialization, for evidence does not unequivocally support this inference. Consider, for example, a recent study of 1811 student political leaders in American high schools, a sample that might be expected to be consistent in its attitudes toward school and politics. Yet "student attitudes towards the wider political system are different from their focus on the high school system" (Grove et al., 1974–75, p. 421). To be alienated from high school does not insure political alienation, nor vice versa. The hidden curriculum of education is not necessarily the hidden curriculum of politics (Takei and Kleiman, 1976, pp. 381–400).

To summarize: the hidden curriculum argument too often presents itself in a non-falsifiable guise; portrays the hidden curriculum as more authoritarian than it is; assumes unwarrantedly that the hidden curriculum is entirely unpalatable to students; assumes with equal lack of warrant that the hidden curriculum is effective; misconstrues the connections among social class, the "demands" of a capitalist system, and the hidden curriculum; and, finally, incorporates an as-yet-undemonstrated transfer of values from school to the political realm. It thus fails to explain the school's apparent weakness in the transmission of democratic values.

Why these blind spots in a theory that holds such promise? Schooling is a combination of educational content and organizational context—what is treated as knowledge and how knowledge is treated. Critics of the school have rightly drawn our attention to the primarily authoritarian context of education, but they have wrongly assumed the impotence of democratic content in such a setting. Democratic values become *more*, not less, important in an authoritarian context, for the conflict between these two elements of schooling triggers the full range of paradoxes that we find in American democratic education. Neither democratic socialization theorists (who overemphasize content) nor hidden curriculum theorists (who overemphasize context) have yet explored this tangle of tensions fully. This now becomes our task. Ultimately we will argue that democratic values in American schools not only weaken the educational enterprise but also, curiously enough, turn democratic socialization in America into unforeseen, unsatisfactory, and unprofitable directions.

The Basic Shape of Schooling

If the school's weakness in transmitting democratic values cannot be explained solely by reference to the hidden curriculum, what then? My approach to the problem addresses two questions: First, what role does the hidden curriculum play in the organization of schools? Second, how does the American school attempt to fit democratic values into the hidden curriculum? I believe that the school's adaptation fails because the intersection of the hidden curriculum and democratic values delegitimizes the school's socialization efforts. The first step in the argument is to identify the hidden curriculum's place in the school's organizational order. I call this conjunction of hidden curriculum and organizational pattern the "basic shape" of schooling.

Mary Metz goes far towards describing the basic shape of schooling when she writes, "Public schools have a paradox at their very heart. They exist to educate children, but they must also keep order. Unless the children themselves are independently dedicated to both these goals, the school will find that arrangements helpful for one may subvert the other. Yet to sacrifice either for the other is to default upon a school's most fundamental responsibilities. . ." (1978, p. 243). The school's problem of order is acute, because schools bring together a large, heterogeneous body of cognitively and emotionally immature, but physically active young people who are required to be in school, and who once there are made to behave in ways that may run contrary to their lives and propensities outside of school. Thus, for example, the same students who compete against each other for the scarce goods of excellent grades play cooperatively when the school day ends. Keeping order under conditions like these obviously presents difficulties. It is this task which the hidden curriculum undertakes.

Of course, the hidden curriculum may also perform other tasks, such as preparing students for stratification by merit in large, impersonal, hierarchical corporations and government agencies. Despite much debate, however, the connections between these external functions and the hidden curriculum remain undemonstrated. We need not explore the issue ourselves, however, for the pressures we have identified *within* the school are more than sufficient to account for the hidden curriculum.

I should emphasize that the school's demand for order is an organizational, not an educational, imperative. Theoretically at least, organizational flexibility may further educational goals, although at the cost of effectively managing and controlling students. This potential hiatus between learning goals and organizational order creates a structural fault line running through the school, a point of recurrent weakness around which much of importance in the school's life revolves.

The basic shape of schooling, therefore, is determined by the school's need to persuade students that there exists a connection in practice, if not in theory, between order and learning. Since order is the special province of the hidden curriculum it is the practices of the hidden curriculum in particular that must be shown to contribute to the learning of relevant educational material. The basic shape of schooling, therefore, consists of a continual search for educational rationales to stabilize the hidden curriculum (cp. Bernstein, 1971, pp. 202–31).

Teachers and administrators enjoy custody over these educational rationales. Both the power of the teaching profession as a whole and the independence of the individual classroom teacher depend upon the public's acceptance of the teacher as an authoritative repository of educational knowledge. Although teaching in America lacks the status of older professions, the school's oft-noted resistance to public pressure (Peterson, 1974, p. 350; Zeigler and Jennings, 1974; Hawthorne, 1978, pp. 362–67) rests on a claim that is substantially identical to that of fully established professions: namely, the possession and application of specialized expertise acquired with difficulty (Meyer, 1977, pp. 54–76; Fisher, 1971–72, pp. 322–36). Teachers claim expertise both in some specialized, useful subject and in effective teaching methods. Teachers usually emphasize one of these two types of expertise. Elementary school teachers must rely for their autonomy primarily on methodological claims, for most middle-class lay persons have a command of elementary school subject matter. By contrast, high school teachers can rely more heavily on their access to specialized knowledge. Still, taken as a whole, both the teacher's independence and the teacher's claim to status rest on the same foundation: the right to dispense useful knowledge over which they alone exercise jurisdiction.

As professionals, therefore, teachers command a series of educational formulae with which to legitimize the hidden curriculum. However, because teachers must work in a school, they have an interest in ensuring school order. Thus the interests of schools and teachers converge. The needs of both can be accommodated only by persuading both students and the public that the social order of the school is an educational necessity. In this way, the strictures of the hidden curriculum gain legitimacy.

Of course, every profession is faced with the problem of justifying its "standard operations procedures." Note, for example, the efforts of doctors and lawyers to relate fee-for-service billing to the quality of medical or legal practice. For their part, educators have elaborated an extensive ideology to justify the hidden curriculum. Knowledge, they claim, is by its very nature complex, sequential, and cumulative. It cannot be acquired haphazardly, hurriedly, or out of order. From this rationale flow such ordering practices as curriculum standardization, age-grading, and careful scheduling of course material. Educators also maintain that advanced knowledge is difficult, and therefore that only a few students can learn the most demanding truths. Therefore, the school must screen, test, and grade its students, a task the hidden curriculum undertakes. Finally, because the teacher commands knowledge to which the student is only an aspirant, it is only right that the teacher enjoy authority over the student, as embodied in and enforced by the hidden curriculum.

Standardization, testing, selection, age-grading, the sequencing of content, legitimate teacher authority—these are the heart of the hidden curriculum. Every aspect of it is justified—either directly or indirectly—from the teachers' educational rationales. This is especially true of those aspects of the hidden curriculum students find most vexing: controls over demeanor and behavior in school. Why must students be quiet in the halls? Answer: because noise would prevent the quest for knowledge. Knowledge and quiet, the hidden curriculum teaches, go together. Why must class periods be 50 (or 30 or 20) minutes long? Answer: this uniform period (whatever it is) is optimal for student attention to material. Learning, the hidden curriculum teaches, must be broken up into short identical chunks of time. Why must there be examinations? Answer: how else can each student be appropriately placed in the hierarchy of knowledge? Knowledge, after all, *is* hierarchical, the hidden curriculum contends. Why must students be graded? Answer: student achievement must be acknowledged and students placed in appropriate "tracks." Knowledge must be dispensed prudently to qualified students and not squandered on unqualified ones. According to the hidden curriculum, advanced knowledge is akin to salvation, theoretically available to all, but in practice limited to the deserving initiate (Apple, 1978, pp. 367–88).

This enforced symmetry between the structure of educational knowledge and the exigencies of the hidden curriculum is not without serious costs. Some teachers convince themselves that ideas which cannot be made to conform to the hidden curriculum are unworthy of serious consideration. In this way the hidden curriculum comes to shape the structure of educational knowledge, rather than being a neutral tool (Metz, 1978, pp. 36–57).

Equally important, however, is the impact on students. At the outset of most courses, students are ignorant of most—if not all—of the material, and therefore cannot evaluate the educational content on its merits. But they are amply familiar with the hidden curriculum, for they have encountered the school's demand for order since their first cautious step into kindergarten. So they naturally assume that if the hidden curriculum in a classroom is in place, the subject matter must be legitimate and worthwhile. Even brief relaxation of the hidden curriculum may indicate to them that the material being presented is not "serious," is just "fun," or calls for ridicule. It is not surprising, therefore, that students think the natural sciences and mathematics "hard" and "important," for in these disciplines teachers impose the hidden curriculum strictly. By contrast, students consider the humanities and social studies less demanding and less worthwhile because in these fields formal controls are often conspicuously and deliberately suspended (Metz, 1978, pp. 125–33, 57).

This brings me to a key point: teaching political values poses a threat to the delicate conjunction of order and content that makes up the basic shape of schooling. Discussing political values in the classroom invites controversy and division, especially in the United States where a heritage of liberalism encourages citizens to make unfettered political choices from a free market of ideas (Gross, 1977, pp. 194–200). As Americans, we expect people not only to advance their own political views, but also to challenge the views of others. Theoretically, at least, no political values are sacred; all are open to question. In practice, of course, some values are widely enough shared as almost to escape debate; nevertheless, espousing even these consensual values in the classroom provides the occasional disaffected student with a chance to dissent.

And what if controversy does erupt? Among some values there exist no politically acceptable choices. For example, the public school teacher can hardly present religious values as knowledge compatible with the hidden curriculum without imposing such values arbitrarily upon a context which our tradition of church-state separation decrees to be one of legitimate value division. But consider the alternative: arguing that one person's religious values are as good as another's, and that all religions (even irreligions) merit respect. Though defensible within our political tradition, value relativism of this sort hardly contributes to the school's demand for order in the service of an indivisible, unquestionable, single curricular truth. In short, the teaching of political, social, and religious values in American schools constantly threatens to undercut the basic shape of schooling.

Understandably, therefore, both value relativism and value advocacy compromise the teacher's authority in the classroom. Students comply with the teacher's dictates because they believe that teachers possess objective truths helpful to all, not salvation for students lucky enough to espouse the "right" values and excommunication for others.[2] At the same time, teachers cannot permit students to decide for themselves what is true and what is not. Why then have teachers at all? On what therefore does a teacher's power depend? The answer appears to be student willingness to believe that there exists some body of incontestably true, generally useful knowledge which the teacher—and only the teacher—commands. Neither value advocacy nor its opposite—value relativism—contributes to this belief.

Finally, the school's demand for order favors an atmosphere of impartiality and respect for facts, educational qualities conducive to the teacher's authority and the school's independence. This atmosphere vanishes when value advocacy opens the school to charges of propagandizing. Equally destructive is the value-relativism approach, for there is always the danger that the spirit of relativism may spread uncontrollably into the teacher's privileged realm of true knowledge. The teacher's self-interest demands that answers to educational questions be construed as either right or wrong. Thus, the math teacher "knows" that there are indisputably correct answers to math problems, just as the English teacher claims to be able to tell "good" grammar from "poor" grammar (Postman, 1973, pp. 86–95), "real originality" in writing from "pure sensationalism." Why should the teacher jeopardize not only the school's demand for order but also his or her own authority by adopting an "every value is equally acceptable" attitude and possibly forfeiting the opportunity to make vital distinctions in the sphere of knowledge? Knowledge and the professional autonomy it conveys must be kept free of value relativism, on the one hand, and political proselytizing on the other.

Thus the context of public education in America conflicts with the context of democratic political socialization, and the school finds itself pulled in contrary directions when it is required to socialize democratic values. Nevertheless, we persist in making just this demand of schools. Let us therefore explore our second question: how does the school adapt to its political socialization requirements? What happens when the school is asked to reconcile its demand for order with the necessity of teaching democratic values?

Bending Schools Out of Shape

Two democratic values of particular, though hardly exclusive, importance to Americans are popular sovereignty and political equality (Dahl, 1956). Americans conceive of popular sovereignty as a means of preventing concentrations of power from estranging leaders from followers, corrupting leaders, and ultimately paving the way for political repression. Elections, public opinion polls, ombudsmen, and a free press are all instruments with which we try to insure popular sovereignty. Americans conceive of political equality as the roughly equal distribution of political power among ordinary citizens, a goal we attempt to implement by adhering to a "one person, one vote" rule, by enacting and enforcing legal barriers against discrimination, by unclogging channels of interest group formation and expression, and, of course, by abolishing all legal entitlements to power and status (Tawney, 1931, Ch. 3). As these examples indicate, our commitments to political equality and popular sovereignty are real, not rhetorical.

Teaching these democratic values causes the school difficulty. The problem which the school confronts is that if students were to apply the two values to the school itself, they would conclude that the hidden curriculum is illegitimate, for the hidden curriculum stratifies students according to competence, elevates teachers above students, and asks students to think of subject matter as a series of settled truths, not as a set of conflicting values open to popular choice. In school, students are not equals, the authority of the teacher is assumed to be unquestionable, and truth is never a "value" which wins the most student votes, but rather a body of ideas which is objectively correct by external standards of evidence or agreement in the keeping of teachers. In short, popular sovereignty and political equality challenge the school's demand for order and the teacher's claim to knowledge.

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[2] I wish to stress that I am here speaking of the way knowledge is *perceived*, not what it actually is in terms of its truth value. Elsewhere I have written on the latter subject (Merelman, 1976).

Schools could keep these democratic values within the basic shape of schooling, but only by dint of singular intellectual gymnastics. Teachers would have to argue that democratic values can be reasoned about in more or less "sophisticated" and "intelligent" ways (thereby legitimizing grading); that democratic values can only be understood by the usual curricular devices of sequencing and pacing; that democratic values should be confined to politics, and not implemented within the school; and, finally, that political values should be subjected to the same rules of logical argument that teachers enforce in other subject fields. In essence, the school would have to argue that reasoning about democratic values constitutes the school's special province, and that such reasoning must be kept separate from the implementation of democratic values, which is the polity's concern. Teaching democratic values within the basic shape of schooling would thus require schools to exempt political reasoning from the egalitarian messages which democratic values convey. The schools would have to disentangle political reasoning from democratic values, and then argue that schools, as the custodians of superior reasoning, should be exempt from the democratic values they are propounding to students (Entwhistle, 1971).

The difficulty of explaining, justifying, and actually carrying out this task prevents its being undertaken. Few American schools try to teach democratic values as intellectual challenges. Nevertheless, educators cannot escape the necessity of teaching democratic values in some form. What alternatives to real intellectual discourse do they adopt? The answer, I think, is that patterns of educational *practice* serve as substitutes for the discussion of challenging democratic ideas, but only in such a way as to deligitimize the schools' socializing function. To explain this argument we must turn our attention to popular sovereignty and political equality not as parts of curricular content, but as aspects of school practice (Boyd, 1978, pp. 577–628.

A good example of how educational processes teach democratic values involves the recruitment of social studies teachers. Low recruitment standards, lax training procedures, and permissive retention policies provide a lesson in popular sovereignty at least as significant as anything that occurs in the classroom. Schools do not teach popular sovereignty by presenting it as a complex value requiring disciplined classroom reasoning, but by tolerating low standards of teaching which diminish the social studies teacher's claim to knowledge and expertise. The teacher's mediocrity reduces the gap between teacher and student, and thus becomes a practical lesson in citizen competence to judge leaders. The continual demonstration of teacher mediocrity substitutes for the complexity of popular sovereignty as a political idea. I thus propose the following hypothesis: the greater an American school system's commitment to transmitting the value of popular sovereignty, the greater will be its tendency to employ teachers just bright enough to claim competence—thus protecting the profession's claim to autonomy—but not so bright as to deserve unquestioned power over the student. Poor teachers demonstrate to students that they need not fear intellectual authority. Thus, every American school needs its poor teachers as well as its good teachers. The derision the barely competent teacher evokes is counterbalanced by the students' growing confidence in themselves which undoubtedly augments the practice—if not the understanding—of popular sovereignty.

Of course, there are sound economic explanations for the paucity of excellent teachers. Yet a purely economic explanation does not suffice, for we must ask ourselves why we permit economic constrictions to channel the flow of competence away from public education. In other important walks of life economic considerations are not permitted to stand in the way of excellence. The answer, I think, is that limited teacher competence in social studies fits our propensities to teach democratic values tacitly in practice rather than explicitly in challenging curricula.

Although this sounds heretical, even perverse, I am in earnest. The evidence suggests that American public school teachers comprise a distinctly mediocre intellectual reservoir (Werdin, 1972, p. 124). Study after study shows that the grade point averages of future teachers fall below the averages of their peers (Study Commission on Undergraduate Education and the Education of Teachers, 1976, p. 48).

Significantly, intellectual mediocrity is particularly common among teachers of social studies, the subject area where, if my argument is correct, mediocrity *should* be centered.

Additional support is provided by the fact that we consign many of our least gifted teaching minds to the elementary school, where the tender age of the student already drives a wedge between teacher and pupil; where, therefore, a warm manner, personableness, and the balm of nurture can prevent the teacher-student gap from becoming a chasm; and where too much intelligence on the teacher's part would simply underline the competence gap between teacher and student. In the interest of illustrating popular sovereignty in practice to elementary students we employ and retain many quite ordinary subject-matter teachers who specialize in the teaching of "social development" and "human relations skills," skills which bring them closer to their students. These teachers treat the class as a large family whose concerns the teacher shares. By contrast, because we value academic competence more highly among older students we can tolerate splitting off teachers from the lives of their mature students. Among older students, therefore, we jettison the familial atmosphere and tolerate status inequalities. The polar case is college, where only the rare undergraduate feels as personally close to a professor as most elementary school students do towards their teachers. In sum, we do not teach popular sovereignty to young people as a complex problem of political reasoning, but we compensate by illustrating the value in educational practice.

If the concept of popular sovereignty appears in the limited quality of our teachers, the concept of political equality emerges in our grading practices. The idea of political equality conflicts with the need teachers feel to protect the sanctity of knowledge via the teaching and grading of students. Most teachers believe that imparting as much knowledge as one knows, as well as one can, to as many students as one can reach, sooner or later requires that potentially invidious distinctions among students be created. Such distinctions conflict with the egalitarianism contained in the political equality norm. But to teach "less well" than one can is not only to violate one's own conscience, but also to compromise the knowledge base of teaching itself.[3] What is to be done?

A dilemma of this magnitude defies simple solution. Schools have developed a pattern of coping, however. In brief, most teachers make certain that they convey roughly the identical form and quantity of knowledge to a majority of students in any classroom. This practice solves a number of problems. It assures that only the bare minimum in any class are permitted to fail and, therefore, that only this bare minimum risk being subordinated to their peers on the basis of their knowledge. Likewise, it assures that only a bare minimum will be able to move rapidly towards demanding material, thus making certain that real success is sufficiently uncommon as not to dishearten the majority of students.[4] The majority in each classroom thus remain equal. The impact of egalitarianism upon the curriculum thus fosters the presentation of "standard packages" of subject matter carefully calibrated to the presumed abilities of a classroom majority, packages which therefore fail to distinguish markedly among students in terms of either interest or incentive. Educators rationalize this practice by arguing that the standard package is knowledge "appropriate" for students of a particular age. In this way a set of standard packages becomes a curriculum, buttressed by IQ scores, reading levels, and a panoply of supportive theory. More formally, we may define an American public school curriculum as that body of knowledge which a majority of age-graded students can understand equally well, and which this majority shares as a community of equals. In a society where political equality is a dominant value, knowledge must unite, not divide people. These observations should make more comprehensible the oft-criticized uniformity and standardization of mass public schooling, and also help us understand why public schools so adamantly resist the reforms critics have for so long advocated (Oliver, 1976; Sarason, 1969). The standardized curriculum of the American public school is too valuable a network of cultural compromises to be abandoned in the interest of a reformer's conception of "better education."[5]

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[3] Lortie (1975) points out an interesting adaptive solution to the problem. The teacher focuses on one or two pupils whom he or she has reached. This solution protects competence while denying the relevance of knowledge for stratifying an entire class. I suspect most professions resort to the same tactic, e.g., the psychiatrist who helps one spectacularly depressed patient out of his misery, the social worker who succeeds in helping one delinquent girl to a better home life, etc.

[4] Might this pattern not be responsible for the short shrift programs for educating the "gifted" have received throughout the nation? See G. I. Maeroff (1977).

An interesting illustration of my argument is the dreary fate of recent laws requiring students to prove their academic competence before moving to a higher grade. Many of these laws originally defined competence so restrictively that the majority of students in a class might well have failed, a failure perhaps justifiable on intellectual grounds, but hardly consistent with the need the school feels to practice equality. "Too much competence" among students is as politically unfeasible as "too much competence" among teachers. Predictably, therefore, initially stiff competence standards have been relaxed in order to accommodate the modest abilities and potentialities of most students. Slowly, like a phoenix from the ashes, the standard curriculum has resurfaced, thus permitting us to practice—if not to teach well—the value of equality (*New York Times*, March 26, 1979).

I have identified two ways by which educational practice adjusts itself to major American democratic values. But despite the school's best efforts, these values cannot be restricted entirely to practice. They must appear somehow as subject matter. Here the teacher confronts another dilemma. From the standpoint of analytic knowledge, popular sovereignty and political equality are political values, not elements of factual truth consistent with the teacher's claim to authoritative knowledge. The problem becomes particularly acute when we realize that the better and more critically such values are discussed, the more controversial they become, and, therefore, the greater the risk they pose to school order. Is there a resolution to this problem?

The resolution the school adopts is that educators distort and simplify these values in ways which appear to fit them within the corpus of authoritative knowledge. Teachers accomplish this metamorphosis by obscuring the logical distinction between political facts and values (a possible reason is contained in Jaros, 1968, pp. 264–95). In Lortie's study, for example, most teachers admitted that they did not know there is a logical hiatus between factual statements and normative propositions (1975, pp. 111–13). Their ignorance is fortuitously self-serving, for it permits them to portray such potentially dangerous values as political equality and popular sovereignty as if they were already matters of accepted fact embodied in practice by the existing school—and political—order. And teachers read so little beyond the most pragmatic aspects of their craft that most can be assumed never to confront the fact-value distinction (Mour, 1977, pp. 397–401). Indeed, many have had only minimal college course-work in the fields where they wind up teaching (United States Office of Education, 1969, p. 41). Such work in the social sciences would obviously help them distinguish between facts and values. The teacher thus becomes a smuggler of values, producing students who react with surprise and bewilderment when, in college, their instructors ask them for the first time to make the fact-value distinction (Perry, 1968). At this late point many students "know"—the standard curriculum package has told them so—that democracy is the "best" form of government, that the United States is a democracy, and that political equality and popular sovereignty are both good in theory and present in practice. It becomes hard for these students to imagine that words like "best," "good," and "democracy" may have various, contestable meanings. Yet their miseducation in this respect ought to be understood as a logical consequence of the way American schools attempt to promulgate democratic values.[6]

To summarize: American schools adapt to demands that they transmit the democratic values of popular sovereignty and political equality by deemphasizing the academic competence of their teaching staffs, by setting their grading standards at levels which insure that a majority of students perform acceptably, if perhaps poorly, and by glossing over the difference between facts and values in politics. In short, American schools adapt by reducing the quality of education.

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[5] It is here that my view diverges somewhat from that of critics like Michael Katz, who portray school rigidities almost entirely as organizational or economic, rather than cultural problems.

[6] Of course, not all students can be expected to confuse facts and values. For example, in the Jennings-Niemi study approximately half of the high school seniors investigated rejected the proposition that the American political system should be extended to other countries (Jennings and Niemi, 1974, p. 65). Yet the interpretation of this datum is by no means straightforward. It is perfectly consistent for students to believe the United States the "best" political system on earth, and yet not to recommend the system to other countries on grounds that these countries do not satisfy conditions which would be necessary in order to make them too "best." Thus, the fact that many students think highly of American government, yet do not advocate its extension is not necessarily anomalous, nor is it a serious argument against the idea that students think American democracy an embodiment of the "best."

Yet this costly adaptation has its ironies. Poor teachers who apply uniformly mediocre standards to distorted social studies ideas are hardly capable of legitimizing the hidden curriculum. And, to repeat, order in school depends upon the hidden curriculum's appearing necessary to good education. Therefore, because the practices I have described threaten the legitimacy of the hidden curriculum, they also impede school order. Even the average student soon realizes that what little is demanded in social studies classes can be accomplished without a hidden curriculum of order. As a result, students resist an ordered hidden curriculum in the social studies, and when they are successful in their resistance—as Metz shows they often are (Metz, 1978, pp. 125–26, 244–45)—they take their success as confirming their initial insight that the social studies are not really "serious" after all. If the social studies were serious, wouldn't they require a serious hidden curriculum? Thus a vicious circle is drawn, a circle in which the school's attempt to accommodate democratic values turns back self-destructively on the school itself.

If this argument is correct, it would follow that in a school system where democratic political socialization does not pose so serious a threat to the hidden curriculum or to the teacher's power, teachers should be able to maintain the distinction between political facts and values, and genuine, critical debate about democratic values should become an accepted and effective part of the schooling process. An example of such a school system is that of France, where teachers feel a level of security and competence uncommon in the United States. It is significant, therefore, that in France

> there is very little attempted control over the ideas which pupils should express. In fact, an integral part of the system is an overt attempt not to restrict or influence the ideological options open to the individual. Thus teachers in the humanities and social sciences present their subject matter in as objective a manner as possible. . . . In addition, teachers try to inculcate within their students a critical bent of mind. These tendencies are clearly illustrated in the French school exercise called *explication de texte,* in which the pupils must analyze and then criticize a given passage in a work selected by the teachers—what to do and how to do it are precisely defined, but the student is given total freedom of expression. The attempt not to restrict the philosophical options open to the child is apparently successful, since the belief systems of the pupils gradually become dispersed all along the ideological spectrum (Schonfeld, 1976, p. 25).

Of course, the school itself may not be solely responsible for the ideological dispersion Schonfeld describes. Schonfeld also ignores the highly restricted modes of expression allowed to French students. But note that in the French case the school's demand for order and the teacher's claim to knowledge are overtly asserted in the process of teaching democratic values. The result is an educational pattern precisely the reverse of that which American schools adopt. The French teach democratic theory; we teach democratic practice.

The confabulation of facts and values in the American social studies curriculum creates two quite specific limitations on political education. First, teachers who pretend that values are matters of settled fact seem unlikely to imbue such values with passion or conviction. To have the proposition that democracy is a good thing presented as if it were equivalent to the proof that 2 + 2 = 4 is to remove from the concept of democracy its singular emotional force. Much damage is therefore done to the value transmission process. After all, people will only internalize their political values deeply and cleave to them intensely if they feel a necessity to confront others who espouse opposed values, to argue for their own, to think through their beliefs, and, through these exchanges, to explore the many applications, implications, and ambiguities of their political lives (Shaver, 1977). The American social studies curriculum provides few such opportunities. As a result, democratic values rarely become deeply rooted in the student's mind. Perhaps this tepid education helps account for the well-known American reluctance to apply abstract democratic values to concrete situations of choice (Jackman, 1978, pp. 303–25). In this case, practice makes imperfect.

At first students face this problem together. But bright students soon think their way through the ruse, and conclude that what the school has presented as fact is actually a complex, confusing matter of debate and personal commitment. Therefore, it is to bright students that the school is most disillusioning. And so, in every school system we turn many of the best students—indeed, many of our future intellectuals—against the educational enterprise (Friedenberg, 1963, Ch. 8). Perhaps this is the most damning critique of all: that the American school's adaptation to the teaching of democratic values causes it to alienate many of the most promising students in each new generation. The school thus forfeits the respect of that one group of future citizens who should be its natural allies.

Conclusion

The limitations of the transmission of democratic values in American schools are the price we pay for the strain between the school's demand for order and the egalitarian norms embedded in American political culture. The result of this conflict between order and equality creates the characteristic American cultural pattern I have described, a pattern in which forms of political and social conduct come to be elevated above political ideas and values (Tapper, 1976).

But the school is not the only place where this cultural strain manifests itself. Comparable problems emerge in the family, where a structure of traditional authority confronts the child's demand for equality (Henry, 1963, pp. 127–47), and on the job, where the traditional power of the boss confronts the skilled worker's demand for greater power. Thus it may be that in this article we have stumbled upon a cultural strain intrinsic to the entire American political enterprise.

The American cultural adaptation—to substitute action for comprehension—is damaging, for the derogation of knowledge and ideas it entails may prepare the ground for the anti-intellectualism so often noted in American political history (Hofstader, 1963). Must schools continually fail to arm us with those modes of understanding we need in order to understand our own political practice? If so, we are doomed to share a kind of culturally patterned political innocence that cannot but recreate itself from generation to generation. Perhaps, unawares, we deny ourselves intellectual tools that could help us more surely fashion our own political destinies.

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Comment on Richard Merelman's "Democratic Politics and the Culture of American Education"

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Richard Merelman is one of the more talented essayists of our discipline. His previous reflections in the areas of power, political socialization, and the politics of education have been penetrating, insightful, and even provocative. The present essay falls within this fine tradition. One can applaud with enthusiasm the application of a sensitive intellect to such a cosmic problem as the teaching of democratic values in the school system. As with all good essays, however, this one raises as many, perhaps more, questions than it answers.

The first and fundamental question concerns the necessary assumption that American education in fact makes little or no contribution to the development of democratic values amongst its charges. Merelman supports this contention primarily by citing research that shows relatively little observable impact on students from teachers, the curriculum, or the school as a whole. Especially because I had a hand in some of the research efforts to which he alludes, I am not going to quarrel seriously with this argument—though here more than is usually the case one wants to utter the obligatory, "more research is needed, we need longitudinal designs, the cumulative impact may be immense, we're looking at the wrong things," and so forth.

But even granting the validity of the research, does it really provide evidence for the accusation of failure on the part of the schools? The answer must be "no" on several counts. Simply because it is difficult to show specific effects does not necessarily mean that democratic values are not being instilled. Comparison with the studies of occupational and income achievement are instructive. Investigators are hard-pressed (as Merelman notes) to come up with substantial *net* effects from particular schools, educational practices, curricula, and the like. But no one seriously argues that most people would be literate or capable of accomplishing an extraordinary number of things if they had no formal education. The evidence is overwhelming that education is a very strong predictor of occupation and income.

The analytic and theoretical difficulty has been trying to determine *how* education performs this task, especially in the seeming absence of strong school (socialization) effects. One resolution has been to look upon schooling as a certification process, with varying amounts and types of schooling entitling one to certain subsequent opportunities over a lifetime and building up a set of widely recognized role expectations (Meyer, 1977). From a socialization perspective it has also been acknowledged that schools tend to have a common, minimal floor of educational content, thus ensuring some commonalities in outcomes. Another socialization argument has been that education produces propensities and dispositions that are evoked later on. In any event, the contention is not that education, per se, has no impact.

Now a straightforward application of this line of reasoning to the realm of schools and the teaching of democratic values would be amiss. Nevertheless, there are some striking parallels. There is a mountain of evidence showing that in adult populations education ordinarily bears a strong relation to what theorists typically describe as democratic values. After conducting a massive secondary analysis of survey data drawn from the 1949–1975 period, Herbert Hyman and Charles Wright concluded, "The large, lasting, and diverse good effects on values found in this study, coupled with the very large, pervasive, and enduring effects in heightening knowledge, receptivity to knowledge, and information-seeking documented in our earlier study [1975] establish that formal education has long been an important force throughout America in molding *character* as well as intellect. Our findings are bound to surprise the reader who has come to accept the continuing, widespread, and severe criticisms of educational institutions. . ." (1979, p. 61, emphasis added).

As with occupation and income, it is not clear to what degree this is a consequence of socialization, certification, or a combination of the two. Does more schooling in this country lead to more exposure to and internalization of democratic values or does it help slot people into subsequent social, economic, and political roles where these values have a better chance to flourish and be reinforced? One is a direct effect, the other more indirect. Both probably operate. As in the case of occupation and income, however, it seems extraordinarily unlikely that the relationship involving education is spurious, resting wholly on antecedent causes. To say, then, that the schools do not generate democratic values would at the very least require an explanation of why it is that education is such a strong correlate of subscription to democratic values.

Additional reservations may be raised about the basic assumption of the article. Offhand, I would have thought Merelman would demonstrate the ineffectual character of education by referencing results from studies of school children revealing their abysmal lack of democratic value orientations. That is the strategy employed by critics of American education with respect to the "three R's." Criticisms of this sort have generated such movements as the return to "basics" and competency testing. Results in the areas of citizenship norms, e.g., those generated by the National Assessment of Education Progress Surveys (Greenbaum, 1977), might have proved equivocal. By reading them with the proper set of eyeglasses, one could at least use these results to buttress the claim of education's failure. Work more specifically in the much-researched area of political socialization might also have been exploited to undergird the claim.

But this tack was not taken. Rather, we are asked to accept failure as a working assumption and along with it much implicit baggage of no mean consequence. For example, if the schools do not successfully instil democratic values, does this mean that the adult populace is also undemocratic? Or that adults magically become more democratic after leaving school? Or that other mechanisms are at work providing preadults with democratic values that the schools are unwittingly doing their best to strangle? The further questions raised by the imputed failure of the schools are perhaps more intriguing than the initial question of *why* the schools fail.

Demonstrating a shortfall in democratic values in an absolute sense is admittedly difficult, for it is vexing trying to decide when enough is enough. Another strategy, then, would have been to take the comparative approach. Are the products of American education less imbued with democratic values than those in other Western countries? If the American students show up poorly, then Merelman would have strong grounds for asking why American schools are failing in their mission. Yet that tack is not taken either. Had it been, there is no assurance that his working assumption would have been supported. The bulk of what survey evidence exists is ambiguous and fraught with the customary perils of cross-national research on political values (e.g., Gallatin and Adelson, 1970; Torney, Oppenheim, and Farnen, 1975; Japanese Youth Bureau, 1973; and Allerbeck, Jennings, and Rosenmayr, 1979). Nevertheless, perhaps a case could have been made, thereby helping to document the existence of a problem.

Merelman's contention about education's failure may or may not be correct, though I would argue that the weight of the evidence in conventional terms is not on his side. My point is that this critical assumption, necessary to the remainder of the thesis, is simply made too casually. Removing or weakening this foundation stone results in the collapse of the entire edifice.

But what if his beginning point is a correct one? Certainly many observers would agree with it, or at least with a less blanketing view that the schools are falling short, even if they are not failing completely. Why is this so? Merelman apparently accepts the characterization of the hidden curriculum but not the devastating consequences it is said to have on stifling the acquisition of democratic values. Assertions that the hidden curriculum is the important one in schools are frequently trotted out, especially by left-leaning critics of the educational system. It is too pat a diagnosis, of course, and stands up to empirical examination no better than do wholesale critiques of the manifest curriculum. Critics on the right—not plentiful in social science circles but quite abundant at the grass-roots level—feel they have as much to complain about. They believe that the old hidden curriculum, with its virtues of hierarchy, task orientation, and differentiation, has been subverted by a new hidden curriculum of student interference, vapid goals, and homogenization. Merelman brilliantly demolishes the hidden curriculum explanation for the schools' asserted failure to transmit democratic values.

I would like to expand that critique in one respect. I have always been puzzled that some observers see childhood political socialization as somehow derivative, that growing children simply generalize from family, school, and secondary groups to the realm of government and politics. Yet differentiation is as much a part of cognitive development as is generalization (Connell, 1971). Children rather early on begin to grasp the difference between the rights and duties of the (adult) political citizen and those associated with being a family member or student. They soon learn that the president and the teacher necessarily govern in different fashions. The evaporation of the happy coincidence between esteem for one's father and the president captured in the early days of socialization research stands as one of the more dramatic illustrations of the fact that there is a political world out there serving as an object for learning in its own right.

One could raise a minor caveat here in terms of what is poured into this vessel called the hidden curriculum. What starts out as a portrayal of hierarchy in relationships turns into "standardization, testing, selection, age-grading, the sequencing of content, legitimate teacher authority—these are the heart of the hidden curriculum" (Merelman, 1980, p. 325). That is, everything except sheer subject matter content seems to be flowing into this vessel of the hidden curriculum. Never mind that its features have been around for a century or that they characterize in greater or less degree virtually all modern education systems. Indeed, they have traditionally been more characteristic of European systems with their rigid separation by academic performance and the accompanying social segregation. In any event, one wonders if the hidden curriculum has actually gone unrecognized all this time.

After showing the deficiencies of the hidden curriculum as an explanation for the purported failure to transmit democratic values, Merelman nonetheless resurrects the concept. He argues that the hidden curriculum is vital to the organization of schools and that there is an inevitable conflict between the hidden curriculum and the teaching of values. In treating the basic shape of schooling as resolving the paradox between educating and keeping order (and by order he apparently means the totality of the hidden curriculum rather than maintaining a quiet classroom), Merelman overstates the uniqueness of the school setting by asserting that in school the students "are made to behave in ways that may run contrary to their lives and propensities outside of school" (p. 324). The example given is that between the rampant competition in the school versus the sweet-spirited cooperation after school. Such a simplification ignores the many modes of cooperation in school (group projects in the classroom, athletic teams, musical groups, etc.) as well as the fierce competition often occurring outside the school (vying for parental affection, sibling disputes, the "dating game," one-on-one playground competition, etc.). What is unique about the schools is that they bring together in one small place such a large number of varying appetites, abilities, and motivations. Magnitude, density, and complexity admittedly create more "people-processing" difficulties than those encountered in the nuclear family.

The hidden curriculum is said to be justified by the educators in terms of how it contributes to the learning of educationally relevant material, though in actuality, "the school's demand for order is an organizational, not an educational, imperative" (p. 324). That may be, but it is difficult to believe that the diverse elements of uniform class periods, examinations, grades, and sequential learning are designed solely for maintaining orderliness and stratification rather than for some defensible reasons having to do with the learning and assessment processes. (There is such a phenomenon as a limited attention span, after all.) Slavish adherence to rules and procedures as ends in and themselves is an ever present danger in large-scale organizations. That is not the issue here. Is it *any* hidden curriculum that is being held culpable by Merelman, or is it the particular one (or ones) describing the American school system?

If the concern is about any hidden curriculum, then an appropriate response to the critique is to ask what is unique about the educational system. If a penchant for order infests the educational system, it is a penchant permeating other institutions also. The work world is characterized by stratification, "invidious" distinction according to accomplishment, superordinate-subordinate relationships, rewards based on achievement rather than ascription, systematic evaluation procedures, sequential staging of job responsibilities, and even (coffee) breaks to relieve the daily routine. Teachers and the schools reflect and reinforce these pervasive tendencies. If the properties of education's hidden curriculum interfere with the "real" business of the schools, then we must also begin to view other institutions in a similar light. Perhaps, indeed, we should.

The climax of Merelman's argument is that the demands of maintaining symmetry between the manifest and the hidden curriculum virtually prohibit the effective teaching of democratic values. Popular sovereignty and political equality provide the test cases. It may be, as he claims, that schools have difficulty in teaching such values, but if they do it is scarcely because the whole fabric of the school system would come unraveled. There would be no more reason for a class of fourth graders to demand sovereignty and equality in the classroom than for these same students to make comparable demands of their parents. Merelman seems to make the common mistake of assuming that exposure to certain ideas would make the students view the classroom as the body politic writ small. If even the majority of university students seem content to acknowledge the inherently undemocratic nature of institutions of higher learning, then surely children in the neighborhood school will not seize upon the ideas of sovereignty and equality as grounds for rejecting the need for "order" and the teacher's legitimacy.

Ironically, public school teachers have an ideal vehicle for introducing such concepts as sovereignty and equality in a situationally relevant way. Each public school system, as distinct from the individual classroom, is actually a commonweal organization, very much caught up in the web of popular sovereignty and political equality. Adult citizens, especially parents of school-age children, act on behalf of children in the politics of education, just as they do in other political arenas. Paradoxically, American public school systems are uniquely vulnerable to the manifestations of these twin democratic values in the form of elected boards of education, frequent referenda, active interest groups, and direct citizen access. Thus there is absolutely no inherent tension between introducing these democratic values in the classroom and the students' reflection on the educational process and educational governance.

It is probably the rare teacher who would use the local school system as a focus, for reasons not too hard to fathom. More generally, avoiding taboo topics in the classroom is an early-learned lesson for the novice teacher. Merelman would have us think that not coming to grips with the topic of democratic values is primarily a means of keeping the hidden curriculum intact. Yet "community-wise" teachers realize that values are controversial, values compete with each other. Parents and groups in the community do not always agree about which values should be stressed. A little expressivism in the classroom can be a dangerous thing (Massialas, 1969, Ch. 6). So a plausible case can be made that the failure to teach democratic values, rather than being internally generated as a way of maintaining the basic shape of schooling, is generated in anticipation of external forces.

As it turns out, however, the schools do communicate a perverted form of democratic values. By a sleight of hand Merelman travels from the point of saying that the schools are incapable of *teaching* such values to that of saying that they demonstrate them through their hiring and grading practices. While not disagreeing with his assessment of the social studies teachers, I think we should remember that most of them are handling subject matter drawn from the poorly developed disciplines of history and political science! Still, the mind boggles when asked to accept the inference that students see mediocre teaching as demonstrating that "Americans conceive of popular sovereignty as a means of preventing concentrations of power from estranging leaders from followers, corrupting leaders, and ultimately paving the way to political repression" (p. 326). Nor can we take seriously the proposition of rising mediocrity accompanying greater commitment to popular sovereignty without assuming some marvelous, invisible hand. The argument that grading practices embody the concept of political equality is more convincing, although the concept of *social* equality strikes me as the more appropriate one in this context. Again, however, I resist the notion that students take these little lessons of life and apply them wholesale to the polity.

A contrast to the American system is suggested in the case of France, wherein a critical examination of democratic values is said to occur in the schools. While the French system has many virtues—the teaching of political philosophy among them—the conclusion that the varying belief systems exhibited by French children spring from a critical thinking approach is a non sequitur. However, the introduction of the French example and the last few pages of Merelman's essay led me to conclude that he is not asking why it is that American schools fail to teach the substance of democratic values; rather, it is why the schools do not teach an *understanding* about democratic values. That is, such values as popular sovereignty and political equality are in some way inculcated reasonably well, for as he acknowledges, "Our commitments to political equality and popular sovereignty are real, not rhetorical" (p. 326).

I have argued that if for no other reason than the strong association between education and democratic values we must give the education system some credit for this "real" commitment. What seems to be less well inculcated is any thorough understanding of such concepts, their philosophical underpinnings, the tradeoffs among them, the conditions giving rise to or threatening them, and their translation from belief into action. These are obviously higher-order educational goals, and—like most systems—we do not measure up well in reaching them. On the other hand, I doubt that the failure to reach them derives primarily from an inherent tension between the hidden and manifest curriculum. But Merelman has laid before us an intriguing thesis, one that we can chew on, and one that will certainly challenge educators.

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A Reply to Jennings

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M. Kent Jennings' admirable comment on my article disputes three fundamental points. Jennings first attacks my "critical assumption" that American education fails to socialize democratic values. He next questions my discussion of the "basic shape of schooling." Last, he argues that weaknesses in the school's transmission of democratic values should not be attributed to a conflict between democratic values and the basic shape of schooling. Let me address these problems in order.

Does the School Contribute to the Development of Democratic Values?

I find Jennings' argument on this question curiously indecisive. He accepts my interpretation of extant studies which show few specific school effects on student democratic attitudes. He admits that studies of citizenship norms, such as the National Assessment of Educational Progress, might buttress my case. He even adds that cross-national studies might support me, although he can provide me with "no assurance" on this point (which, I suppose, means he cannot assure me of the contrary either). Given his evident vacillation, I hope I may be forgiven my uncertainty about the strength of Jennings' opposition on this issue.

In any case, Jennings charges me with a crime of which I am not entirely guilty. I specifically say (p. 333), "It is hard to believe the schools are not having *some* impact on democratic values," and I then try to identify the elusive and unsatisfactory nature of this impact. The question, therefore, is exactly in what respects Jennings and I differ.

We differ about what should be accepted as evidence for the school's impact on democratic values. Writing about adults, Jennings states, "To say . . . the schools do not generate democratic values would at the very least require an explanation of why it is that education is such a strong correlate of subscription to democratic values" (p. 334). The logic of this argument escapes me. Can Jennings fairly attack my discussion of schooling, children, and adolescents by dragging in adults, whom I discuss only by implication? Even if adult evidence is admissible, it would seem that the school's limited impact on today's young should give us reason to doubt, rather than to assert, its impact on yesterday's young. The logic of Jennings' strategy places too heavy a burden on his argument.

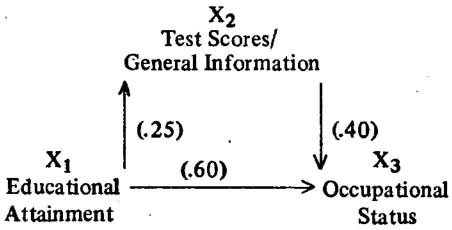
Still, logic aside, educational attainment is often correlated with both democratic attitudes and political knowledge among adults. What should we make of these findings? Despite his admonition that it would be "amiss" to do so, Jennings proceeds to liken these findings to the correlation between educational attainment and occupational success. Jennings should have heeded his own warning, for comparison between the two bodies of data is dangerous.

Initially, the strength of school effects on occupational status exceeds the strength of school effects on political knowledge and democratic values. Jencks et al. (1979, p. 169), updating their 1972 research, report correlations (r) of .60–.65 between educational attainment and occupational status for a sample of Kalamazoo, Michigan brothers. By contrast, correlations (r) between educational attainment and political information average only .20–.25 (Hyman, Wright, and Reed, 1975, p. 157).

These data have relevance for the certification-socialization controversy Jennings mentions. It seems reasonable to assume that the certification effect of education is manifested primarily through education's relation to occupational status, and that the socialization effect is manifested primarily in the relationship of educational attainment to political information. If so, schools seem to be considerably more effective and efficient as certification devices. Jennings compares two processes which differ in both character and magnitude, and pretends they are similar processes of the same magnitude.

Do these conclusions survive statistical controls for social background? They do. Not only do all the data I have reported include background controls, but also when we compare proportions of variance explained (r^2) after partialing out background effects, educational attainment explains 22.3 percent of the variance in occupational status among adults (Featherman and Hauser, 1978, p. 259), as opposed to only about 4 percent of the variance in adult political knowledge (Hyman, Wright, and Reed, 1975, p. 157).

Finally, certification also enjoys causal priority in the school's functioning. Take occupational status as a dependent variable, for example. Do people get good jobs because of the high level of knowledge which advanced schooling imparts to them or because they happen to have been credentialed by staying in school a long time? Although I have no estimate of the relationship between scores on Hyman's measures of information and adult occupational status, we can employ a surrogate: the correlation between adolescent achievement test scores and occupational status. Assuming that adolescent test scores decently predict adult general information, we can sketch the following causal model:[1]



"Eye-balling" is always dangerous, of course, and this little causal model should be viewed with due caution. Nevertheless, the certification path (X_1-X_3), which does not take knowledge into account, seems much stronger than the socialization path $(X_1-X_2-X_3)$, which does take knowledge into account. Thus, contrary to Jennings' implication, the socialization and knowledge transmission function of American schools apparently does not compare in magnitude or causal priority to the certification function of schools. Jennings ought not to borrow from strong findings in order to buttress weak ones.

In addition, Mary Jackman (1978, pp. 303–25) has recently attacked Jennings' assertion of a strong relationship between educational attainment and belief in democratic values. Jackman shows that well-educated white adults are as inconsistent as less-educated whites in their application of democratic values to race relations, that the well-educated are as unwilling to practice tolerance toward blacks as the less-educated, and, finally, that the well-educated are no more able than the less-educated to overcome their dislike of blacks in order to apply democratic standards to racial questions. Jackman concludes, "The well-educated are more likely to have genuinely 'learned' abstract democratic principles, but that learning is relatively superficial" (1978, p. 322). Thus, even Jennings' factual assertion is unconvincing.

Describing the Basic Shape of Schools

Jennings advances three arguments on this subject: first, that the school is not unique in experiencing a conflict between organizational order and institutional purpose; second, that schools include cooperation as well as competition, while competition is as common outside the school as within; and, third, that the hidden curriculum performs educational functions. On all three counts he is unpersuasive.

I agree that all large organizations suffer in some form from the conflict I describe. But I am talking about schools, not large organizations in general. Why Jennings thinks the general case can be used to discredit the particular instance escapes me.

As for Jennings' second contention, let us consider the examples of cooperation he cites. In each case—group projects, athletic endeavors, and musical performance—competition in some form remains dominant. Group projects pit some students against others in the pursuit of grades. Athletic encounters pit school against school in the pursuit of honor. And musical groups are usually ranked by seniority, by difficulty of music performed, and by the juried quality of musicians admitted. Most important, the schools mandate, encourage, and reward these group competitive endeavors. By contrast, consider the examples of extra-school competition Jennings describes. Two of them, competition for parental attention and sibling rivalry, are specifically *discouraged* by parents, who regard them quite correctly as detrimental to family life. One, playground sports, is generally considered by participants to be game-like and to carry few serious long-term consequences (hence, the absence of cheerleaders, the friendship among competitors, the regular exchange of positions and handicaps, the nonexistence of fans and referees). Only the "dating game" closely resembles competition within the basic shape of schooling. On balance, therefore, Jennings' argument does not seriously dispute my description.

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[1] Correlation between X_1 and X_2 estimated from Hyman et al. (1975, p. 157); correlation between X_2 and X_3 estimated from Jencks et al. (1979, p. 113); correlation between X_1 and X_3 estimated from Jencks et al. (1979, p. 169). I should stress that in this model I am considering Hyman's, Wright's and Reed's category of general information, not political information. Adolescent test scores would seem a better estimate of the former than the latter. In addition, it seems more plausible that general, rather than political, information would aid in the attainment of occupational status. I would like to thank Robert Browning and Herbert Kritzer for their advice on these rough statistical estimates.

Equally unconvincing is Jennings' contention that the hidden curriculum (which he mistakenly equates at this point with the basic shape of schooling) serves educational functions. True, students do have limited attention spans, but they do not all have the same limits, and each student's limits differ from subject to subject. Therefore, uniform class periods must serve organizational functions, for they can meet the educational needs of only a lucky handful of students. This fact is so obvious that even educators realize it, for many continue wistfully to lament and pursue the "individualized instruction" which the basic shape of schooling effectively prevents them from attaining. Additionally, because demands for order resembling those of the hidden curriculum permeate large organizations other than schools (as Jennings points out), it seems reasonable to think of the hidden curriculum primarily as an organizational, not an educational, tool.

The Conflict Between Democratic Values and the Basic Shape of Schooling

Jennings argues that community pressures, not the schools themselves, are primarily responsible for the school's weakness in democratic education. Again, however, there are logical problems in Jennings' assertion. If he is right that pupils will decline to apply the values of American democracy to schools, might they not also decline to apply ideas learned in school to the American polity? The differentiation principle in cognitive development—which Jennings thrice approvingly cites—hardly supports an argument that schools can produce a democratic citizenry.

Jennings also repeats his disconcerting habit of advancing an argument which he specifically disavows. First he asserts that teachers can use school district politics as a model for the teaching of democratic values. In the next breath, however, he argues that only the "rare" teacher would actually do so, for public school politics involves "taboo" topics. Although I know such topics exist, I am sure that Jennings is not referring to the topics I discuss, popular sovereignty and political equality, for these must certainly be among the democratic values he assures us educated Americans endorse and advocate. Indeed, if so many educated Americans really to subscribe to democratic values, as Jennings contends, it becomes even harder to understand why such values do not play a greater role in school curricula. After all, educated Americans are disproportionally represented on school boards.

What is particularly strange is that Jennings himself, along with L. Harmon Zeigler, discovered as early as 1974 that school politics are *not* really democratic and that schools have little to fear from public participation, protest, and controversy. In their exhaustive and revealing study of school governance (1974), Zeigler and Jennings report that school politics are frequently noncompetitive even in elective districts (p. 34); that few school board issues involve programs and curricula (p. 44); that "the competitive milieu of school board elections is not fierce" (p. 91); that there is little interest group input into school decision making (p. 99); and that superintendents frequently frustrate public intrusion into school governance (pp. 139, 166–67, 185). They conclude that public school politics hardly conforms to a democratic model (p. 243). Thus, Jennings' own findings show us that public passivity in school politics is the rule. How then can the public be held responsible for preventing democratic socialization in the schools?

Jennings contends that American schools have nothing to fear from democratic values among their students. I doubt that many teachers would agree, given their growing anxiety about their physical safety and their dependence upon maintaining classroom control in order to gain tenure and promotion. Under these conditions even one "troublemaker" trying to assert equality and popular sovereignty forcefully in the classroom might seem a threat. It only takes a few aggressive, intelligent, alienated students to make the incompetent teacher's life a hell. And surely many teachers remember—even if Jennings does not—that the most articulate of their students in the 1960s did much to upset the basic shape of schooling.

Finally, I do not understand Jennings' disbelief that students learn to suspect authority when they are subjected to the caprices of temporary leaders (teachers) who lack intellectual competence. Nothing so makes a student feel skeptical, assertive, and disrespectful than the realization that his or her teacher cannot back up power with intelligence. Intellectual parity with the incompetent teacher is a powerful "little lesson of life," at least as powerful as any lesson learned from the banal and poorly argued democratic rhetoric such teachers usually parade. The point of my article is that neither democracy as idea and ideal nor education as the sharpening of minds is properly served by relegating everybody to the same dead intellectual level in the name of democratic practice.

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Explaining the Variable Utility of Disjointed Incrementalism: Four Propositions

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Much of mainstream organization theory has been concerned with the implications for organizational design and policy process of high levels of uncertainty or complexity in task environments. Decentralization, disjointed incrementalist decision strategies, and quasi-market coordinative mechanisms have been advanced as rational responses to the complexity of most problems in the socio-political sphere. This article presents and illustrates four conditions which reduce the relative utility of this approach as a means of coping with uncertainty. The propositions are shown to be implicit in the logic of "muddling through," and are used to help explain/predict the evolution of relatively centralized and planned organizations in certain types of complex task environments.

For the myriad and complex worlds in which organizations operate, no one best way of designing organizations or of making decisions can ever be specified. One of the great contributions of modern organization theory is to have established that traditional attempts to trace the one maximally efficient organizational design were misconceived. Thus, after sustaining an elaborate argument for the advantages of incremental decision making, decentralization, and market-type coordinative mechanisms, Charles Lindblom reminded his readers that "disjointed incrementalism" and "partisan mutual adjustment" had *not* been shown

> in some general sense [to be] superior to central decision making.... The issue of general superiority is in any case a spurious one, for questions about the merits of alternative methods arise in various specific contexts; hence it is in these contexts that one needs finally to evaluate them (1965, pp. 293–94).

The purpose of this article is to develop an analytic capacity for comparing the relative utility of incrementalist/market strategies for making and coordinating decisions versus those relying on planning, centralization, and comprehensive analysis, in different kinds of "contexts." My analysis is designed to assist organization theorists in their efforts to match decision strategies more efficiently to problem types and to explain the "rational" evolution of relatively synoptic decision strategies in certain kinds of complex problem areas.

Decision Strategies for Coping with Uncertainty

For the last 20 years cognitively oriented organization theorists have ascribed overwhelming importance to notions of complexity and uncertainty as independent variables in the explanation of organization structure and policy practice.[1] Of particular importance in establishing the dominance of this view has been the work of James D. Thompson, Herbert Simon, Charles Lindblom, and Martin Landau. The large body of literature that their work has helped to generate has led to an expectation that skillful organization managers operating in complex social, economic, and political task environments will:

- –abandon absolute rationality norms and computational decision strategies because of inability to predict outcomes and/or agree on ultimate objectives (Thompson and Tuden, 1959);
- –construct simple models of problems in the context of which "boundedly rational" responses can be developed (March and Simon, 1958);

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[1] For contrasts between the cognitive approach to the study of organizations and the "human relations" approach, see the debate between Herbert Simon and Chris Argyris conducted in the pages of the *Public Administration Review* (1973). I use the term "cognitively oriented" instead of "rational man" (as used by Simon and Argyris) to avoid implicit reference to earlier "rationalist" theorists such as Frederick Thompson and Luther Gulick.

- substitute marginal, disjointed experimentation for a priori policy analysis, and sensitivity to feedback for coordinative planning (Lindblom, 1959, 1965; Braybrooke and Lindblom, 1963);
- adjust organization structure and resource deployment so as to reduce uncertainty and isolate its effects (J. D. Thompson, 1967);
- tolerate redundancy, duplication, and overlap and treat errors as opportunities to learn rather than signals of failure (Landau, 1969, 1973).

The work of these theorists was used with enormous effectiveness by opponents of planned program budgeting. It has provided a solid intellectual foundation for the judgment that the utility of hierarchically organized centralized decision-making structures is radically reduced in complex problem areas.[2] To cope with confusion over both means and ends, organizations are urged and expected to decentralize, respond amelioratively to signals of error, and abandon serious attempts to plan.[3] Recent trends toward decentralization and more consultative authority styles have been interpreted as evidence supporting this view. Management science textbooks, often under the rubric of "contingency theory," are translating it into a new conventional wisdom (LaPorte, 1975, pp. 352–55).

However, complexity is not the only salient independent variable and uncertainty is not the only issue. Although Lindblom and other cognitively oriented theorists have acknowledged that not all complex problems are equally susceptible to incrementalist solutions (March and Simon, 1958, p. 204; Lindblom, 1964, p. 157; 1965, pp. 293–94; 1975, p. 52; 1977, pp. 65–89), very little systematic attention has been devoted to the identification of conditions pertinent to the task of "rationally" matching more or less incremental or synoptic decision strategies to different types of complex problems (V. Thompson, 1971, p. 11).

The Variable Utility of Disjointed Incrementalism

Because Charles Lindblom is the best known and most widely understood of the authors cited, I shall use his models of disjointed incrementalism and partisan mutual adjustment to develop four propositions linking the character of complex task environments to the relative utility of more or less decentralized, more or less incremental, more or less epiphenomenal decision strategies. In his classic article "The Science of Muddling Through" (1959) and in *A Strategy of Decision* (1963) and *The Intelligence of Democracy* (1965), Lindblom integrated the insights of Simon, Thompson, and others into a positive strategy for *coping* with complex problems—problems that are too difficult to *solve* in the face of disagreement over preferred outcomes and a dearth of powerful social theory. Just as March and Simon (1958) approved of simple search procedures by administrators lacking the resources necessary for comprehensive scan and analysis of their task environments, so Lindblom suggested disjointed and incremental experimentation on organizational margins to avoid goal displacement and encourage adaptive innovation without risking organizational survival (Lindblom, 1959; Braybrooke and Lindblom, 1963).

Drawing on neoclassical market theory, Lindblom went on to argue that coordination under conditions of uncertainty might better be achieved epiphenomenally, i.e., as the byproduct of autonomous efforts by various actors to achieve their objectives through ad hoc accommodations with other actors. Through "mutual adjustment," competing decision makers within an organization, or partisan interest groups on a more macro level are said to match the allocation of resources to aggregate preferences much more satisfactorily than centrally sponsored attempts to achieve coordination through standardization, schedules, and plans.[4]

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[2] For a sophisticated review of conceptual issues raised for this school of organization theory by the problem of complexity as well as useful bibliographies, see LaPorte (1975).

[3] Collective goods theory can be interpreted as a branch of cognitively oriented organization theory which identifies hierarchy and centralized planning as efficient responses to special forms of preference related uncertainty (created when external economies or diseconomies are complemented by high transaction costs and small numbers) (Williamson, 1975, pp. 31–33).

[4] Lindblom (1965) uses the term "partisan mutual adjustment" (PMA) to characterize the market-like way in which coordination is achieved among incrementalist actors in the American political arena. Bargains struck and policies adopted, changed, and discarded at the national level reflect the exertions of a multiplicity of actors pursuing their individually ordered preferences with the social, economic, and political resources at their command. In later works,

The four propositions stated below specify four kinds of problems for which the utility of disjointed incrementalism and mutual adjustment, as techniques for coping with complexity, must be discounted relative to the utility of more synoptic, centrally coordinated strategies. Identification of these categories is based on a deductive analysis of disjointed incrementalism and the implicit assumptions on which its utility, and the utility of mutual adjustment coordinative mechanisms, are based.

The relative utility of disjointed incrementalism as a strategy of decision and mutual adjustment as a coordinative mechanism must be discounted to the extent that:

1. *Intersecting causal chains in an organization's task environment link issues of different orders of magnitude of salience for that organization.*
2. *The complexity of an organization's task environment is non-decomposable.*
3. *An organization lacks a redundancy of resources.*
4. *Power (defined as the ability of an actor to manipulate his environment in pursuit of his goals) is unevenly distributed in an organization's task environment.*

It must be stressed that by delineating non-epistemic conditions under which the utility of disjointed incrementalism and partisan mutual adjustment must be discounted relative to that of centrally planned decision making and coordination, I am making a substantial, but *limited* claim. I am not claiming that a reduction in the utility of decentralized decision strategies means that a highly centralized, planned strategy would therefore work more satisfactorily. As my illustrations will indicate, that is sometimes the case; but precisely what is claimed is that under the specified conditions the comparative advantage of "muddling through," as a decision-making and learning strategy, decreases. I am not claiming that organizational actors will be able or even inclined to discover whether these conditions are present in their task environments. Nor am I claiming that, if they do measure their task environments and note the presence or absence of these conditions, they will respond appropriately, by centralizing, analyzing, and planning, or decentralizing and experimenting, as the case may be. However, according to an evolutionary logic, there will be a long-term tendency for organizations to move in the "appropriate" direction along an incremental——synoptic continuum. This is to be expected, even if managers of organizations have no cognizance of these factors, simply because particular types of organizations will be relatively more successful in particular kinds of complex task environments.[5]

What follows is explication and illustration of the four propositions stated above.

PROPOSITION 1. *The relative utility of disjointed incrementalism as a strategy of decision and mutual adjustment as a coordinative mechanism must be discounted to the extent that intersecting causal chains in an organization's task environment link issues of different orders of magnitude of salience for that organization.*

According to Lindblom, unanticipated and unwelcome consequences of small changes in policy are more likely to be remediable than those precipitated by large changes. An organization faced with considerable uncertainty is therefore advised to proceed incrementally, under norms which lead to the expectation of error and the likely necessity for post facto remedial action to cope with unanticipated consequences.

However, unanticipated consequences of actions (large or small) are likely to be more susceptible to remedial action in some uncertainty-laden task environments than in others. In most task environments linear increases or decreases in the values of variables generate smoothly continuous changes in the values of causally connected variables. Insofar as this is true, unwelcome and unanticipated consequences of errors are likely to be susceptible to remedial action involving resource commitment commensurate with the purpose and scope of the original program. On the other hand, insofar as task environments are characterized by sharp discontinuities or thresholds in the values of variables, or by the intersection of causal chains linking issues of different orders

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especially *Politics and Markets* (see pp. 247–60), Lindblom uses other terms to refer to essentially the same processes, e.g., "epiphenomenal," "market oriented," "inter-active processes," "political processes," and "mutual adjustment."

[5] Only to the extent that performance efficiency is correlated, *in the long run*, with organizational survival will the "rationality" of matching organizational form to appropriate types of problems find empirical expression. It is precisely because in nature efficiency is so well rewarded, that the various behavioral and physical characteristics of different species can be seen as "rational" responses to their habitats (i.e., their "task environments").

of magnitude of salience for the organization, then post-facto incremental adjustment is relatively less useful. For if thresholds are crossed, even incrementally, or if incremental actions in an unimportant sphere have significant impact on matters of vital importance, then the value of resources necessary to remedy unanticipated and unwelcome consequences is almost certainly likely to be *in*commensurate with the scope of the original program and the value of the resources originally deployed (Schulman, 1975, pp. 1367–69; Dorfman and Dorfman, 1977, pp. 35–37; Goodin and Waldner, 1979, pp. 6–9).

One can appreciate the importance of threshold effects in reducing the utility of incrementalism and mutual adjustment by comparing the costs of using tax or fee schemes versus direct regulation to manage specific pollution problems. Incrementalist efforts coordinated by quasi-market mechanisms would be the preferred decision strategy for an agency responsible for the abatement of sulfur dioxide pollution. In general, linear changes in the amount of sulfur dioxide released daily into the atmosphere result in levels of pollution that change linearly in response to daily emissions. Since sulfur dioxide pollution is regarded as undesirable in roughly direct proportion to its level, incremental increases in a trial-and-error search for optimum emission levels will not trigger massive costs. It would, relative to other pollution problems, be wasteful for such an agency to spend a large proportion of its resources on a priori attempts to establish optimum, or "safe," standards for smokestack emission, or to specify the types of "scrubbing" equipment that various kinds of factories would be required to install.

Thus it would seem reasonable for a national environmental protection agency to allow agency representatives in different cities (with different sources of sulfur dioxide emissions and different local utility functions regarding industrial productivity and clean air) to experiment in order to discover that standard (or tax schedule) for emissions suited to their particular jurisdiction. Moreover, it would be unreasonable for such an agency to enforce specifications on the type of "scrubbers" to be used by factories for the removal of sulfur dioxide from the wastes emitted from their smokestacks. Rather, by creating markets in pollution or through tax or fee schemes, it should encourage individual factories to experiment with different abatement techniques. As long as the deleterious consequences of the failure of an individual factory's experiment with a new scrubber design increase in a smoothly continuous fashion with the extent of the failure, then the air quality agency can afford to adopt highly disjointed, incremental strategies for learning how to abate sulfur dioxide pollution. While some factories' innovations will fail, the success of others will produce new knowledge useful to all.

In contrast, the utility of disjointed incremental decision strategies must be discounted for the abatement of radiation pollution by nuclear power plants. Nuclear power plants emit low levels of radiation in the normal course of operation, and although this radiation may harm the health of plant workers in the long run, its consequences are assumed to vary incrementally with length of exposure. In any case, they involve directly only small numbers of people. However, in the event of a coincidence of appropriate malfunctions and/or human errors, nuclear power plants are susceptible to meltdowns and explosions that could disperse highly radioactive material over a wide area. In other words, a pollution control agency charged with the control of radioactivity emissions by nuclear power plants would face a task environment characterized by certain sharp discontinuities in the levels of damage consequent on incremental error. A trial-and-error, incrementalist approach to organizational learning is based on a presumption that errors will be made, but that, overall, the value of the knowledge thereby gained for the organization will exceed the value of the resources needed to remedy the results of those errors. This presumption is less warranted when one is considering nuclear power or, for example, the disposal of utterly toxic chemicals. In these cases a priori analysis and the central enforcement of standardized procedures become more feasible and worthwhile.

Indeed, it will be noted that although factories are encouraged to experiment in the design of sulfur dioxide scrubbers, safety equipment and procedures are centrally specified and enforced for nuclear power plants. Probably more could be learned quicker about how safer and cheaper nuclear reactors could be built if individual power plants were permitted to experiment by trial and error with innovations in equipment and safety regulations. But this incrementalist, disjointed strategy for organizational learning assumes that the consequences of error will fall within the same order of magnitude of scope and salience as the scale of the resources devoted to the innovation. This is not the case in the nuclear power task environment, where intersecting causal chains link issues of very different orders of magnitude of salience, *viz.* holding down costs of building and operating safe power plants, and protecting hundreds of thousands of local inhabitants from death by radioactive poisoning. In this context, therefore, the relative utility of disjointed incrementalism must be discounted.[6]

This most general formulation of Proposition 1 requires some explication. Disjointed incrementalism is partially advocated as a strategic adaptation by organizational actors to the absence of a cardinal utility function, i.e., to the inability of members of the organization, or of its constituent parts, to agree on goals and priorities. An organization using this strategy would advise its lower-echelon administrators to engage in the subdivision and factoring of problems "in an unrestricted variety of ways, without regard to lines along which interdependency can be minimized" (Braybrooke and Lindblom, 1963, p. 120). For some organizations the problems confronted by different sub-units involve values/goals of the same order of magnitude of salience (as seen from the perspective of the organization as a whole). In such situations unanticipated and deleterious consequences will be no more costly no matter which problem is confronted first. This is an important condition for the relative utility of *disjointed* incrementalism, since one of its features is that only minimal effort is made to agree on the relative importance of different problems confronted by the organization. However, if one of the organization's problems is of much greater salience than others, then serial, disjointed decision making is much riskier. The presence of thresholds in task environments makes it more likely that changes in causally interconnected variables will involve issues of different orders of magnitude of salience.

PROPOSITION 2. *The relative utility of disjointed incrementalism as a strategy of decision and mutual adjustment as a coordinative mechanism must be discounted to the extent that the complexity of an organization's task environment is non-decomposable.*

The usefulness of disjointed incrementalism as a strategy for learning and policy improvement depends in part on the capacity of loosely linked organizational units to remedy the consequences of error, or at least to respond appropriately to negative feedback. An organizational unit that depends on negative feedback for error correction operates on the implicit assumption that signals received from the unit's task environment are discrete responses to actions taken by the unit. Accordingly, task environment responses to error correction are continuously monitored to determine the appropriateness of compensatory behavior and the further necessity for changes in policy. My first proposition focused on complex task environments in which feedback from discrete errors may overwhelm responsible units. My second proposition focuses on complex task environments in which discrete feedback is unavailable to responsible organizational units.

In a seminal article entitled "The Architecture of Complexity," Herbert Simon sought to suggest, without providing a formal definition of "complexity" itself, that most complex systems or structures were characterized by "hierarchy." Simon used this term to refer to complex phenomena which could be understood as successive sets of subsystems whose interrelationships were less intense than the relationships among the units of which they were constituted. He then suggested the "decomposability" of a hierarchically complex phenomenon as a measure of the extent to which the behavior of individual subsystems could be accounted for, independent of directly or indirectly related subsystems (1965, pp. 63–65, 69–73). It appears that several of the conditions which have been identified as resulting in the failure of incrementalist techniques for coping with complex problems, such as "sleeper effects" and the prevalence of synergistic causal chains, can be interpreted as characteristic of complex task environments that are less "decomposable" than others.

Non-decomposable complex problems are characterized by causal chains elongated in space or time. In these task environments as compared to those which are more decomposable, changes in one variable are more likely to trigger significant changes in other than the local subsystem, changes less readily apparent to the responsible organizational unit. Confronted with relatively "non-decomposable" complexity, an incrementally oriented unit, even if it anticipated the likelihood of error and monitored its task environment carefully for signals indicative of the need to change policy, would be less able to appreciate the need for remedial action. This is because the responses of its own task environment (including the actions of other organizational units) would reflect the impact of implemented policies less accurately than if the variables it manipulated operated more independently from "distant" subsystems. In other words, in relatively non-decomposable task environments, errors are less likely to result in negative feedback that can be monitored by the unit responsible for the error. Therefore one must discount the utility of disjointed incrementalism as a strategy of decision in complex task environments which are relatively non-decomposable. Conversely, to the extent that the complexity facing an organization is relatively decomposable, then the attractiveness of such a strategy increases compared to a priori forms of analysis and centralized modes of coordination.[7]

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[6] On December 15, 1978, two articles concerning the Environmental Protection Agency appeared in the *Wall Street Journal.* Their juxtaposition neatly illustrates my point here. One article described EPA proposals for abating air pollution such as auctions and other "market-style" mechanisms for the buying and selling of "rights to pollute." The second article concerned new comprehensive rules to be centrally enforced by the EPA for the disposal of utterly toxic chemicals.

To illustrate the effect of causal chains elongated in space on the utility of disjointed, incremental decision strategies, consider two societies, each endeavoring to use and protect water resources as efficiently as possible. Assume that both societies are trying to cope with an equal water-land-population ratio and that both confront similar degrees of uncertainty as to how the water resources of their societies could and should be exploited. The water resources of one society, however, are divided among many separate watersheds, no one of which contains more than 10 percent of the water resources of the society as a whole. In contrast, 90 percent of the second society's water resources are concentrated in one watershed, for example, a large river.

The attractiveness of a disjointed and incremental strategy of decision for the first society's national water commission is clear. Because of effects on the ground table, or on regional economic development and consequent population shifts, water policies implemented in one watershed will have some impact on other watersheds. In general, though, the links between the water resources of one region and those of other regions are neither comprehensive nor intense (i.e., the complexity of the system is relatively decomposable). Thus new knowledge gained by lower-echelon sponsored experiments in water regulation or exploitation in one region may be disseminated by the national commission to other regions, but the consequences of errors by regional or local authorities do not ramify rapidly through the whole system. Remedial action by those authorities, involving resources which correspond in scale to those committed to the innovation, is likely to be possible. Disjointed incrementalism as a means of coping with and learning about the complex problems associated with the exploitation of water resources would seem eminently suitable in this first society.

However, the second society's national water commission confronts problems of relatively non-decomposable complexity. It must discount the utility of a disjointed and incremental strategy of decision regarding the exploitation of its water resources. Especially if the society's water resources are concentrated in one large river, local errors in pollution abatement, for example, could lead to contamination of the vast majority of the society's water resources. Because water resources in one half of the country (downstream) are powerfully affected by the state of water resources upstream, an experimental trial-and-error approach by local units of the national water commission could result in massive costs to the society as a whole. Moreover, because of the spatial elongation of the causal chains involved, the lower echelons responsible for sponsoring innovations upstream would not be in a position to experience the consequences of their errors directly, nor, even if they could, would they have the resources necessary to remedy their mistakes. Therefore, the water commission of the second society should, logically, discount the utility of feedback-dependent decision strategies for water exploitation relative to centrally enforced standardized procedures, and simulation, deductive theory building, or other a priori forms of comparatively synoptic analysis.[8]

The same point concerning the implications of decomposable and non-decomposable task environments is illustrated by the recent relaxation in federal guidelines for research into recombinant DNA. Severe restrictions were placed on scientific experimentation with recombinant DNA because of fears that elongated causal chains existed such that incremental errors in local laboratories could result in widespread cancer epidemics. The perception was that local laboratories, and therefore local experiments, could not be isolated from their surroundings: that this task environment was spatially non-decomposable. But research conducted at the maximum safety facility at Fort Dedrick, Maryland, showed that, in fact, bacteria containing recombinant DNA molecules have a "general lack of infectivity."[9] The Department of Health, Education, and Welfare's relaxation of restrictions on recombinant DNA experimentation can be understood as a reasonable response to the evaluation of this complex task environment as more decomposable than previously thought.[10]

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[7] The proposition may be framed alternatively as follows: "Complexity" is commonly defined as some combination of differentiation, rate of change, and interdependence. Complex task environments are relatively more resistant to incrementalist coping techniques to the extent that their complexity derives from the interdependence of changing and differentiated components, rather than from the rate of change or extent of differentiation per se. For a discussion of problems attending the operational definition of complexity, see LaPorte (1975, pp. 3–39).

[8] For concrete illustration of this hypothetical example, see Wittfogel (1957, pp. 162–63). Wittfogel explains the evolution of highly centralized despotic regimes in ancient river valleys as "rational responses" to the distribution of sources of humidity available to those societies.

The decomposability of complex task environments may be reduced by causal chains elongated in time as well as in space. If so, errors made by individuals or organizational units at one point in time may trigger chains of consequence whose unpleasant aspects are not apparent for months, years, or even decades. These "sleeper effects" deprive the incremental decision maker of prompt negative feedback. Indeed the steady, stable signals received may well encourage continuance and even marginal expansion of the programs in question (Goodin and Waldner, 1979, p. 7).

The logic of discounting decentralized incremental decision making when sleeper effects are present is illustrated by increasing federal regulation of carcinogenic materials. Increased reliance on centrally promulgated rules and standards in this issue area is directly linked to a growing belief in the importance of sleeper effects in the etiology of cancer. According to a recent study of asbestos-related cancer,[11] a 30-year latency period "can intervene between initial exposure to asbestos and the development of asbestos-related cancer." The study attributed 15 percent of annual cancer deaths in the United States to the heavy exposure of American workers to asbestos since the beginning of World War II. Because of the time-elongated causal chain between exposure to asbestos and the appearance of malignant tumors, workers and factory managers cannot depend on the scale of immediate unpleasantness to determine how much exposure to asbestos in the workplace they should countenance. The Environmental Protection Agency has, appropriately, installed a central toll-free hotline in Washington to answer questions concerning asbestos exposure.

Finally, Paul Schulman's study of the National Aeronautics and Space Administration provides an excellent example of how synergistic relationships among causal chains elongated in both time and space severely restrict the utility of disjointed incrementalism and decentralization as a strategy for coping with complexity. Schulman explains the evolution of NASA from a maze of separate smaller agencies into a non-incrementalist centralized organization as, among other things, a rational response to the non-decomposability (he uses the term "indivisibility") of the space exploration task environment:

> The development of a rocket guidance system, for example, demanded simultaneous advances in both computer miniaturization and thruster engine design. The study of acceleration tolerances ... required the construction of large centrifuges in order to simulate the stresses of space flight. Research into designs and materials which would safeguard spacecraft reentry into the earth's atmosphere depended on sophisticated hypersonic wind tunnels and was delayed pending the construction of such devices (Schulman, 1975, pp. 1358–59).

Because synergistic relationships were so prominent in this task environment, even incremental payoffs or signals of error required "comprehensive commitments of personnel and resources" (Schulman, 1975, p. 1362).

In sum, the elongated causal chains that comprise relatively non-decomposable task environments tend to conceal the significant consequences of discrete actions. Under such circumstances the relative utility of feedback dependent techniques for coping with complexity is reduced.

PROPOSITION 3. *The relative utility of disjointed incrementalism as a strategy of decision and mutual adjustment as a coordinative mechanism must be discounted to the extent that an organization lacks a redundancy of resources.*

Both Lindblom and Landau stress that organizations operating according to the norms of disjointed incrementalism and relying on mutual adjustment for coordination among organizational units: (1) encourage the disjointed pursuit of short-term goals by organizational units; and (2) interpret errors by and conflict among organizational units as opportunities for the emergence of better policies for the organization as a whole, not as signals of dangerously wasteful behavior. Indeed, an organization that believes its resources to be relatively abundant compared to the scale of the problems with which it is attempting to cope, can afford to adopt decision strategies which involve neither hope nor possibility of streamlining available resources to pursue direct, immediate, and narrowly "efficient" solutions. However, to the extent that an organization perceives its resources to be meagre and possibly inadequate to cope satisfactorily with the challenges of its task environment, then it will be less useful for that organization to adopt strategies of decision that assume the short-run "waste" of resources. That is, the less redundant are an organization's resources, the smaller the proportion of those resources will it rationally be willing to invest in learning processes that promise marginal improvements in future policies.

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[9] "Gene-splicing Risks Assessed Low" (1979).

[10] "Recombinant DNA Guidelines Relaxed" (1978).

[11] "Cancer and the Workplace: 'A Disaster'" (1978).

For an organization with minimal slack in its budget, the sacrifice of some resources now for a little more knowledge later may prevent it from surviving long enough to apply the knowledge gained. For such an organization, the attractiveness of centrally coordinated plans based on available theory (however weak) increases relative to that of disjointed incremental decision strategies. In the extreme case, where the survival of the organization is immediately at stake, and available resources are perceived as desperately short, it is possible to reject disjointed incrementalism on logical grounds (since it assumes the deployment of substantial resources in a manner which will not be directly and immediately productive). In extremis, only a comprehensively planned and explicitly coordinated commitment of all available resources (however likely to fail) holds out the logical possibility of success.

Consider, for example, the implications of different resource levels for family food budgeting. A wealthy family might experiment in its choice of food purchases, even permitting individual family members, on their own initiative, to buy unfamiliar foods with little regard to their nutritional value or comparative price. This could be part of a sensible strategy for the family to learn more about how to maximize its eating enjoyment. If some purposes replicated those made by others, if some foods purchased were not nutritious or economical, and if substantial quantities were discarded, still the family's diet would likely be full and varied enough to provide minimal nutritional requirements at bearable cost.

To a poor family, however, it is very important to know which foods provide maximum nutritional value at lowest cost. With barely enough money to ensure that its members are properly nourished, the poor family would, rationally, tend to avoid hit-or-miss strategies for spending its food dollars. Instead, budgeting strategies which centralize purchasing decisions, to avoid duplication and to exploit information about comparative prices and the nutritional value of different foods, will be much more attractive.

The same proposition can be illustrated in an entirely different empirical domain. In *Strategy and Structure: Chapters in the History of the American Industrial Enterprise* (1962), Alfred Chandler compares the evolution of major American corporations and explains similarities and differences in organizational design and managerial behavior by analyzing the different kinds of contingencies faced by different corporations at the same point in time, and by the same corporations at different points in time.

Repeatedly Chandler shows how sustained losses and dangerously low profit margins led corporation executives to discard incremental, disjointed, and highly decentralized administrative procedures, in favor of heavier reliance on a priori analysis and comprehensive reorganizational schemes. In the recession which followed World War I Dupont suffered serious financial losses. The ambitious diversification and disjointed growth of the company during the boom of the war years had resulted in an overextension which suddenly threatened the viability of the company. This state of affairs triggered demands within the company for a "dictator" with "absolute jurisdiction over personnel" and "full authority to do what he could to meet the crisis" (Chandler, 1962, p. 104). This was the atmosphere in which a plan for comprehensive reorganization of the company, which had been rejected a year and a half earlier, was adopted (September 1921) and immediately implemented. Similarly, Chandler notes (1962, p. 365) that both Westinghouse and General Electric engaged in "ad hoc structural adjustments" in the late 1920s. In the early 1930s, however, Westinghouse sponsored comprehensive analysis of its administrative problems. Westinghouse turned to "structural planning and reorganization before General Electric because it felt the profit decline resulting from (the Great Depression) more sharply." The near-collapse of General Motors in 1920 resulted from uncoordinated borrowing by subsidiary managers with wide authority. This spurred GM to abandon its highly disjointed strategy of corporate development in favor of a comprehensive plan for the company's reorganization drawn up by Alfred Sloan. Finally, Chandler points out (1962, pp. 223, 351) how both Jersey Standard and B. F. Goodrich adapted to falling profit margins in the early 1930s by *temporarily* abandoning decentralized "multi-divisional" organizational designs in favor of highly centralized functionally specific departments. Several years later, with the return of corporate prosperity and an increase in the redundancy of company resources, both corporations decentralized their authority structures.

The point is not that scarcity of resources necessarily leads or should lead to the adoption of centralized and comprehensive control systems (though the common practice of adopting rationing programs in wartime is instructive), but that the comparative advantage of decentralization and incrementalism for coping with complex problems is based in part on the implicit assumption of a redundancy of resources. Amid relative abundance, resources may be invested in many programs. As mere marginal adaptations, they are expected to fail, or achieve no more than partial success. This is a sensible way to collect insights without risking huge investments in the results of speculative theorizing. But as resources diminish, the probability increases that many *partial* successes may ensure total failure. Resource scarcity is thus a condition which reduces the comparative advantage of trial-and-error experimentation over the separation of analysis and practice entailed in the implementation of comprehensive plans.

PROPOSITION 4. *The relative utility of disjointed incrementalism as a strategy of decision and mutual adjustment as a coordinative mechanism must be discounted to the extent that power (defined as the ability of an actor to manipulate his environment in pursuit of his goals) is unevenly distributed in an organization's task environment.*

Cognitively oriented organization theorists agree that when an organization has powerful theory at its disposal, and when it knows what it wants, it can adapt to changes in its environment through centrally coordinated implementation of analytically derived policies. Disjointed incrementalism as a strategy of decision, and mutual adjustment, as a method of coordination, are suggested by Lindblom (and others) as strategic adaptations to the need for organizational learning when certainty regarding cause-effect relations and value preferences does not prevail. In lieu of a cardinal utility function, the organization (collectivity) establishes tradeoffs among conflicting values epiphenomenally—by permitting competition and bargaining among components (groups) with conflicting value preferences. This approach is also advanced as a strategic adaptation to the incompleteness of knowledge. If powerful theory is not available for centrally coordinated application to changing circumstances, disjointed experimentation by organizational units is advised, based on independently generated theories and situationally developed competencies. These two rationales for incrementalism *cum* partisan mutual adjustment are linked, since

> decision makers become watchdogs for values they fear will be neglected by others, and each decision maker develops great sensitivity to certain lines of consequence and becomes more competent to explore them than others for whom they are incidental to other objectives and values (Braybrooke and Lindblom, 1963, p. 129).

This parallels the role ascribed to the market in economic theory: that of combining individual utility functions and exploiting widely dispersed technological skills for the achievement of an optimal allocation of resources. In the context of both arguments it is suggested that the set of incremental actions, decisions, or non-decisions, that emerge from interactive processes, *constitute* policies superior for purposes of organizational or collective adaptation to those which would likely have been generated by centralized coordination and analysis.

However, the outcomes of mutual adjustment processes accurately reflect the integration of a multiplicity of utility functions, and effectively exploit the quality of theory that is collectively available, only to the extent that all those who have theories and ranked preferences have access to the bargaining arena. If power is unevenly distributed in a society, or in an organization, the preferences of the relatively powerless lack effective watchdogs, and the sets of incremental decisions that result from partisan mutual adjustment are skewed in favor of the rank-ordered preferences of those actors with more power. Furthermore, disjointed incrementalism and partisan mutual adjustment, as strategic adaptations to uncertainty, are based on an assumption that errors of omission or commission by some organizational actors will be rendered less serious because of the likelihood that their actions will be noticed, confronted, and usefully analyzed by other decision makers with different understandings of cause-effect relationships. But an uneven distribution of power, reflected in differential access to important resources and to bargaining arenas, militates against the development and application of theoretical frameworks by the relatively powerless. As a result, widely dispersed knowledge and situationally generated competences are less efficiently exploited.

The Galbraith critique of laissez-faire economics in the United States, most conveniently advanced in *Economics and the Public Purpose* (1973), is based in very large measure on the logic of Proposition 4. Galbraith identifies what he understands as a perverse gap between the society's de facto utility function, as manifested by its pattern of commodity output, and its real utility function (as discerned by Galbraith) reflecting critical needs by masses of Americans for housing, health care, education, etc. Galbraith never takes issue with the neoclassical view of the market as an efficient mechanism for transmuting incrementally expressed value preferences into patterns of resource allocations which correspond to the weighted signals (dollars spent) of consumers. He does challenge the attractiveness of such a decision strategy in an American task environment where power is heavily concentrated in the hands of a small number of corporations, and in which a large minority of Americans, because of their relative poverty, lack the resources to make their ranked preferences felt in the marketplace.

One does not have to accept Galbraith's analysis or his call for comprehensive economic planning to recognize that concentration of overwhelming economic power gives managers of giant companies opportunities for passing on costs to consumers. In other words, given inequality of power, the self-correcting aspects of market mechanisms manifest themselves less effectively, and the utility of the market mechanism (involving partisan mutual adjustment by autonomous producers and consumers) must be discounted as a strategy for coping with the society's macro-allocation problem.[12]

In *The Best and the Brightest* David Halberstam illustrates how, in another task environment, the utility of a mutual adjustment process for adapting to a complex problem was reduced by an uneven distribution of power among the participants in the process—an uneven distribution that interfered with the exploitation of situationally generated competences. According to Halberstam, in the early and mid-1960s American policies in Vietnam were determined incrementally, largely on the basis of a sustained process of bargaining among different foreign policy elites and the bureaus and agencies with which they were associated—for example, McNamara and the Pentagon, the Joint Chiefs and the armed forces, and Rusk and the State Department.[13] Each participant in the decision process brought to it somewhat different value preferences as regarded the stakes of our involvement in Vietnam (e.g., the relative value of military victory versus high casualties, versus domestic tranquility, versus relations with our allies and the Third World). Each participant also came to the problem with a somewhat different theoretical perspective. These perspectives bore the peculiar stamp of the skills and experiences that brought the participants to their respective positions and generated different interpretations of the Vietnam problem. Halberstam (1972, p. 346) makes the important point that, because of the blows inflicted upon it during the McCarthy period, specifically regarding the "fall of China," and because of Dean Rusk's relative weakness as an advocate in interbureaucratic battles, the State Department was unable to play its natural role as an advocate for understanding foreign policy problems in political terms and as a designer of political and diplomatic solutions:

> It was all these factors which allowed State to play such a dormant role in the year 1964, when the political situation in Vietnam continued to collapse and the strength of the Vietcong became ever more evident. When, under normal procedures, it should have been pushing harder than any other organ in the government to come up with political alternatives, when it should have been sending out warning signal after warning signal to the White House about the darkness immediately ahead in the tunnel, saying that the tunnel was getting longer and darker, State simply did not pose deep and probing questions.

Politically oriented analyses of the war were generated at the State Department but they were regularly superseded by military definitions of the problem and its solution (Halberstam, 1972, p. 369). These were advanced with enormous clout and effectiveness by the Pentagon and the armed forces, vigorously represented by Secretary of Defense Robert McNamara and the Chairman of the Joint Chiefs of Staff Maxwell Taylor.

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[12] This is Lindblom's argument in Part 5 of *Politics and Markets,* where he argues that large U.S. corporations have so much power that the results of political interaction, or "partisan mutual adjustment," favor the utility functions of business. Lindblom's argument, translated into the language I am using, is that the distribution of power in the United States is so uneven that the relative utility of epiphenomenal coordinative mechanisms, as opposed to more hierarchical forms of regulation, must be discounted in the economic policy realm.

[13] For scholarly interpretations of American foreign policy as the product of bargaining among organizational subunits pursuing their own special purposes see Graham Allison's application of the "bureaucratic politics" model (1971), and Morton H. Halperin (1974).

I do not wish to imply that another decision strategy, involving more centralized coordination and comprehensive analysis by the president, would necessarily have resulted in a better set of policies toward the war. The point is that the utility of the decision strategy that was employed, to the extent that it did depend on mutual adjustment processes for the correction of error and the exploitation of dispersed expertise, was reduced because of the uneven distribution of power among the participants in the process.[14]

Conclusion

The overall argument I have presented in this essay is that disjointed incrementalism and the epiphenomenal coordinative mechanisms with which it is associated vary in their appropriateness as strategic adaptations to complex task environments. The choice of any decision strategy involves costs. The argument here is that *the costs associated with incrementalist strategies are likely to rise, relative to those that would be associated with more synoptic strategies, to the extent that one or more of the conditions discussed in this essay are present.* In some complex problem areas synoptic strategies will be more efficient as techniques for coping with uncertainty than incrementalist strategies. Accordingly, it has been possible to illustrate my four propositions by considering the evolution of certain organizational structures toward highly planned and centralized designs when faced with "appropriate" types of complex task environments.

However, as I have stressed, my argument does *not* include the assertion that centrally directed plans will necessarily or even probably be more useful than disjointed and incremental strategies in the presence of one or more of these conditions. The point is rather that the utility of disjointed incrementalism and mutual adjustment must be discounted *relative* to the utility of more synoptic strategies.

For these propositions to serve as more than rough guidelines for the designers of organizations, reliable techniques must be developed for measuring task environments, and repertoires of organizational responses must be assembled and arrayed along a continuum from market mechanistic incrementalism to centrally directed comprehensive planning. In this regard significant conceptual problems remain to be solved, problems that lie outside the scope of this essay.[15] These propositions do, however, help to explain the variable evolution of organizational design and the variable success of disjointed incrementalism as a means of coping with complexity.

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[14] For the country as a whole, one might argue, the Vietnam War stands as evidence for the relative disutility of disjointed incrementalism and partisan mutual adjustment in foreign policy. Expensive domestic policies that serve the interests of few and wreak havoc on many, quickly set in motion countervailing political forces aimed at their reversal. Where the harmful effects of policies are borne by those (like the Vietnamese and poor black draftees) with no effective access to the national bargaining process, the self-correcting aspects of mutual adjustment do not so readily manifest themselves.

[15] It may well be, for example, that systematic correlations between gradually more incremental decision strategies and gradually more decentralized coordinative mechanisms will be difficult to establish. In terms of theoretical elegance, it may be possible to reduce the number of propositions by subsuming Propositions 2 and 3 in Proposition 1, though raising the level of abstraction prematurely is not likely to help illuminate the argument's practical significance.

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Bounded Rationality in Budgetary Research

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Two bounded rationality theories of federal budgetary decision making are operationalized and tested within a stochastic process framework. Empirical analyses of Eisenhower, Kennedy and Johnson domestic budget data, compiled from internal Office of Management and Budget planning documents, support the theory of serial judgment over the theory of incrementalism proposed by Davis, Dempster and Wildavsky. The new theory highlights both the structure of ordered search through a limited number of discrete alternatives and the importance of informal judgmental evaluations. Serial judgment theory predicts not only that most programs most of the time will receive allocations which are only marginally different from the historical base, but also that occasional radical and even "catastrophic" changes are the normal result of routine federal budgetary decision making. The methodological limitations of linear regression techniques in explanatory budgetary research are also discussed.

The lure of the bounded rationality paradigm for researchers in organizational decision making has been strong because many of the policy problems such researchers are most fascinated by are characterized by a degree of complexity which exceeds human decision makers' cognitive constriants. Whenever this degree of complexity is present, and whenever selection environments are not so restrictive as to impose unique solutions, the paradigm of bounded rationality suggests that we can improve our understanding by examining the decision heuristics or "aids to calculation" which decision makers use to make sense of their difficult problems. First advanced by Simon (1957), the bounded rationality paradigm encompasses those choice "theories which incorporate constraints on the information-processing capacities of the actor" (Simon, 1972, p. 162). More particularly, such theories emphasize limited alternative sets, systematic search routines, and the cognitive difficulty of marginal value tradeoffs in the face of "incommensurable" objectives.

One prominent application of this paradigm in the area of budgetary research has been the theory of incrementalism. This theory was first proposed verbally by Lindblom (1961, 1970), and was subsequently developed in a budgetary context by Wildavsky (1964, 1975) and by Fenno (1966). As operationalized by Davis, Dempster and Wildavsky in 1966, however, the theory of incrementalism posited that budgetary decision makers simplify very complex allocational problems by relying on a few "decision rules," which can be expressed in terms of linear regressions. Since these incrementalist "decision rule" models were first proposed, their influence on quantitative budgetary research has been remarkable. Variations on the basic Davis, Dempster and Wildavsky regressions have been used to analyze and to replicate budgetary decision processes in 56 and then in 116 federal domestic bureaus and in the Congress (Davis et al., 1966, 1971, 1974), in the U.S. Department of Defense (Stromberg, 1970), in U.S. state governments (Sharkansky, 1968), and in municipal governments in the U.S. (Larkey, 1975), in Britain (Danziger, 1974), and in Norway (Cowart, Hansen and Brofoss, 1975). In all of these applications, regression R^2's have been observed to exceed .9.

Despite the diffusion and apparent statistical success of these incrementalist models, a number of researchers have remained skeptical (Natchez and Bupp, 1973; Williamson, 1966; Ripley et al., 1973; Gist, 1974; Wanat, 1974; LeLoup, 1978b). In this same skeptical spirit, the present article has two purposes. First, the methodological foundations of quantitative incrementalist research are examined and criticized. Traditional time series linear regression of the incrementalist variety, I will argue, is a relatively nonfalsifiable approach for distinguishing among plausible decision process models. In its stead, I propose the use of distribu-

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tional tests based on cross-sectional program allocation change data. This alternative testing procedure is grounded in the modeling methodology of stochastic processes.

Secondly, in a more substantive vein, an alternative bounded rationality theory of budgetary decision making, to be labeled "the theory of serial judgment," will be developed. The perspective to be adopted here is that, despite specific problems with the incrementalist models, the paradigm of bounded rationality remains a useful approach to the analysis of budgetary decision making. Bounded rationality has spawned a rich assortment of alternative decision theories,[1] all of which are consistent with behavioral constraints on information processing, alternative sets, value integration, and the ability to predict consequences. And serial judgment is another such theory, which differs from incrementalism in part because of a more explicit and contextually dependent search process, and in part because of the more ambiguous character of its final choice selection.

Both the theory of incrementalism and the theory of serial judgment are operationalized as stochastic process models which predict the distribution of percent allocation change. These two distinct predictions are then evaluated empirically, using domestic program allocation data from the Eisenhower (F.Y. 1957), the Kennedy (F.Y. 1964), and the Johnson (F.Y. 1966) administrations. The empirical emphasis is on various stages within the executive half of the budget planning cycle.

Process versus Outcome Incrementalism

The incrementalist "Theory of the Budgetary Process," first described and applied to federal budget data by Davis, Dempster and Wildavsky in 1966, emphasized the importance of "aids to calculation" in substantively complex, yet institutionally stable problem environments. Institutional stability was said to engender stable mutual expectations, which markedly reduce the burden of calculations for the participants. Given stable expectations, budgetary decision makers treat each others' decisions as reliable information indices. On the basis of such a reliable "base," decision makers were hypothesized to rely on simple decision rules of the form: "Grant to an agency some fixed mean percentage of that agency's base, plus or minus some stochastic adjustment to take account of special circumstances" (p. 532). The cognitive emphasis on fixed mean percentage change imposed a linear structure to such decision rules. This structural framework was postulated to be temporally stable, except for a few discrete "shift points" in which parameters and/or functional forms changed due to pressure from a variety of exogenous events. Percentage parameters, in turn, reflect past learning about the strategic biases inherent in the roles of advocate and guardian.

Within this linear decision rule framework, Davis et al. (1966, p. 537) operationalized a variety of simple regression equations, each of which embodied a different approach to strategic "gaming." But in 87 percent of the bureau cases, the empirically dominant incrementalist model of executive request behavior was

$$\text{REQUEST}_t = \alpha_1 \text{ APPROPRIATION}_{t-1} + \epsilon_t.$$

And in 80 percent of the bureau cases, the empirically dominant model of congressional appropriation behavior was

$$\text{APPROPRIATION}_t = \alpha_2 \text{ REQUEST}_t + \phi_t.$$

This theory of budgetary decision making, while both influential and straightforward in and of itself, has unfortunately been plagued by confusion generated by the multiple uses of the term "incrementalism" (LeLoup, 1978a). The term "incrementalism" in its broadest form has been used to refer to concepts as disparate as marginal change, linear decision rules, fair shares, stability in advocate and guardian rules, and "the strategy of disjointed incrementalism" propounded by Lindblom (1970).

To clarify the meaning of "the theory of incrementalism" to be analyzed in this article, therefore, I will adopt a convention proposed by numerous authors (Wanat, 1974; Bailey and O'Connor, 1975; LeLoup, 1978; Crecine, personal communication). "Incrementalism" in the descriptive sense refers to a pattern of marginal change in final allocation outcome relative to some base, which frequently is the previous year's allocation outcome. Or, as Davis, Dempster, and Wildavsky themselves put it, "This year's budget is based on last year's budget, with special attention given to a narrow range of increases or decreases" (1966, pp. 529–30).

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[1] Besides Simon's original pioneering work on satisficing (1957), see, for example, Cyert and March's "adaptive rationality" (1963), Steinbruner's "cybernetic and cognitive process paradigms" (1971), Newell's and Simon's hierarchical search heuristics (1972), Winter's evolutionary theory of the firm (1971), and Radner's "putting out fires" (1975).

This marginal change use can be labeled either as "outcome incrementalism" or as "marginality" and will not be the focus of this article.

Incrementalism in the explanatory sense, on the other hand, refers not just to the fact that some reasons for observed marginality are given (Wanat, 1974), but rather to a particular process of decision making which underlies the observed allocation choices. From this explanatory point of view, the crucial process hypothesis embedded in the Davis, Dempster and Wildavsky models is the linear decision rule hypothesis mentioned above. Other aspects of incrementalist theory, such as institutional stability, mutual expectations and strategic calculations, serve the important theoretical function of increasing the a priori plausibility of this formalization. However, it is the linear decision rule aspect which is tested by quantitative goodness-of-fit measures.[2] This hypothesis, therefore, may be labeled "process incrementalism," and will be the focus of this article's analysis.

Viewed in this manner, the decision process models of Davis, Dempster and Wildavsky have a clear affinity with the broader bounded rationality paradigm. Process incrementalism, in this sense, is simply one particular form of the "simple decision heuristics" or "standard operating procedures" emphasized by Newell and Simon (1972) and by Cyert and March (1963). Alternative formal operationalizations of "standard operating procedure" theory which exist in the budgetary literature include Crecine (1969) and Gerwin (1969).

Even with this context of "process incrementalism," however, Davis', Dempster's and Wildavsky's decision rule interpretation of their empirical regression results have been challenged by a number of their critics. Natchez and Bupp (1973), for example, have argued that aggregate bureau level statistics can tap only "massive administrative stability" and completely miss the purportedly more political policy choices which occur at the program level. Williamson (1966) has derived the identical incrementalist "mark up" functional forms from radically divergent optimization principles. Gist (1974) has alleged that incrementalist patterns are artifacts of uncontrollability in agency allocations. Ripley et al. (1973) have shown that appropriation-expenditure linkages underlie incrementalist regression results. LeLoup (1978b) has pointed out that budgetary decision processes appear quite differently depending upon time period measurement, level of aggregation, and dependent variable format. And, in perhaps the most damaging critique of all, Wanat (1974) has demonstrated that the empirical goodness-of-fits of Davis et al. can also be generated by a purely random decision model in which percent cuts or increases are drawn each time period from a Uniform [0, .1] distribution.

Taken individually, the significance of these disparate critiques can be challenged (Padgett, 1978). However, at least two more general themes underlie these arguments. First is the methodological point, to be developed in a later section, that the incrementalist regression findings are very susceptible to multiple process interpretations. Second is a more substantive concern with the rigidity implicit in the temporally fixed, linear decision rule formulation. Linear rules appear to relegate the impact of bureaucratic, political and technical dynamics either to annual "stochastic adjustments" to fixed percent changes or to very rare "shift point" alterations in fundamental program parameters. The dominant image of incrementalist theory is one of a very inertial and buffered bureaucratic system which extrapolates deterministically from t to t+1. For many researchers, an "as if" justification of formal models built upon this image is sufficient. For others, however, the image seems unacceptably far removed from qualitative accounts of what actually transpires within the federal budgetary process (Natchez and Bupp, 1973).[3]

One particular qualitative observation motivating the present research is based upon the Office of Management and Budget and the Department of Housing and Urban Development internal budget memoranda which were available to me.[4] The contextually rich information detail alluded to by Davis et al. does not appear to play, on an annual basis, only the subsidiary role of "stochastic adjustments to take into account special circumstances." Rather, complex information on a wide variety of substantive, administrative, political and economic dimensions appears to be the heart of decision making and of disputes over program allocations. Indeed, in the executive branch at least, one never observes literally a two-stage decision process in which first fixed percentages of base are calculated and then "special adjustments" made.[5]

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[2] Of course, this is not to imply that other aspects of incrementalist theory cannot be investigated within the regression framework. For example, estimated regression coefficients can be interpreted in terms of "strategic calculations."

[3] The question of appropriate criteria for the evaluation of formal models is of course complex and subject to dispute. However, for one articulation of the position that plausible process correspondence is one such criterion, see Simon (1968). For the opposing viewpoint, see Friedman (1953).

[4] The Crecine archive is a rich collection of internal OMB budgetary planning documents and memoranda, focusing primarily on the OMB Office of Budget Review. This archive spans the period from the Truman through the Nixon administrations, and is described in greater detail in Crecine (1977). The Crecine archive was assembled by J. P. Crecine, G. Galloway, M. Kamlet, D. Mowery, J. F. Padgett, and C. Stolp. The Padgett archive was assembled from the

Despite this apparent absence of literal incrementalist decision rules, Davis, Dempster and Wildavsky are justified in their emphasis on the cognitive limitations of budgetary decision makers faced with exceedingly complex and voluminous information. Executive budgetary decision makers do experience great difficulty when confronted with the task of making omnisciently rational tradeoffs across a wide variety of issues and programs, as is indicated by the demise of PPBS. And use of "base" figures as comparative reference points is widespread.

The Theory of Serial Judgment

The theory of serial judgment is an alternative hypothesis about the behavioral structure of program or bureau level federal domestic budgetary decision making. Like process incrementalism, the theory of serial judgment remains in the bounded rationality tradition; however, it implies greater temporal flexibility than the linear decision rule formulation. This alternative theory derives its name from two distinctive features: (1) a sequential search through an ordered set of discrete budgetary alternatives, and (2) a non-deterministic final selection based upon the slightly ambiguous application of "informed judgment."

Like the incrementalist decision maker, the serial judgment decision maker begins the choice process with a fixed reference point or base, which is historically given in the form of prior budget estimates. From this historical starting point, however, the serial judgment decision maker next makes a conscious choice about "direction of search"—namely, whether to search for alternatives representing increased budget levels or whether to search for alternatives representing decreased budget levels. Propensities to search in one direction or the other may be influenced by institutionalized role biases, by aggregate fiscal policy "climate," and by decision makers' opinions about the substantive merits of the program in question. Once serial judgment decision makers start searching, they perceive a number of salient discrete budgetary alternatives, based upon their knowledge of a wide variety of program-specific contextual detail.[6] The distribution of alternatives so perceived will of course be influenced by the program's legal and technical constraints, which are often discussed in the budgetary literature under the rubric of "controllability."

Within this salient alternative framework, the serial judgment process of choice is simple—one just starts cycling sequentially through alternatives encountered along the direction of search, either adding discrete increases or subtracting discrete decreases, until an alternative is encountered which is deemed acceptable both on the grounds of program merit and within the context of fiscal policy constraints.[7] These serial accept/reject decisions are based on the application of informal "judgment," which is the mixture of cognitive predisposition and contextual sensitivity to a variety of disparate programmatic detail. In other words, each

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files of the HUD budget office, and spans the fiscal years 1957–1970.

[5] The only cases of executive decision behavior I know of which might be interpreted within this framework are occasional across-the-board percent cutting exercises, used to generate a list of alternatives for further evaluation. These exercises were observed during the Eisenhower administration, and also during the very last year of the Johnson administration.

[6] The contextual determinants of such salient alternatives are almost too large to enumerate. However, examples include the following: the size of Boston's latest Urban Renewal application, the amount of unobligated balance carry-over from the previous year, the remaining dollars left in congressional authorization, the sizes of new construction starts which could be deferred, the amount of mortgage assets which could be sold at a discount, the dollar implications of inflation in per unit costs, the budgetary cost of projected demographic change in populations eligible for transfer payments, the dollar implications of new presidential or agency proposed legislation, and so forth. The diversity of these contextual cues is one major reason for a stochastic formal representation.

[7] The role of "fiscal climate" is specifically singled out because of the Crecine (1975) finding that aggregate constraints on the overall size of the federal budget are crucial to the structure of OMB and executive budgetary decision making. "Merit" considerations, moreover, are meant to be interpreted broadly to include political, strategic and administrative evaluations as well as substantive policy preferences.

accept/reject decision may be justified by reference to a number of incommensurable and frequently unmeasurable issues which are attached to the program. However on average, the probability of accepting salient alternatives is determined by the decision maker's cognitive predisposition towards the merits of the program and by the decision maker's evaluation of the aggregate fiscal climate. These probabilities can be interpreted as stochastic analogues of "aspiration levels." Final choice is treated as non-deterministic in part because of an assumption that no unambiguously correct budgetary alternative is perceived to exist.

Serial judgment decision theory has its roots in three broader behavioral choice traditions. First, the affinity of this decision process with Simon's theory of satisficing (1957) is obvious. Serial judgment theory presumes that only a limited number of alternatives are considered; it postulates that the first "acceptable" alternative encountered is chosen; and it highlights the notions of search and, implicitly, aspiration levels. Secondly, serial judgment theory is consistent with one of the three heuristics of Tversky and Kahneman for "judgment under uncertainty" (1974)–namely, the heuristic of "anchoring and adjustment." The serial judgment decision maker starts with an historically given initial value, in the form of prior budget estimates, and then systematically adjusts this initial value by cycling through neighboring alternatives, which are taken here to be discrete in character. Finally, the theory of serial judgment builds upon March's and Olsen's emphasis (1976) on ambiguity in organizational choice. Informal judgment is required because omnisciently rational calculations are infeasible. Serial judgment, I argue, is not an unreasonable way for decision makers to cope with federal policy complexities.

In qualitative OMB and HUD budget planning memoranda, serial judgment frequently is manifest either in the form of HUD "priority bands" of program cuts or in the form of itemized OMB listings of program increases and decreases, which are then cycled through and tabulated until some aggregate fiscal target is achieved.

Linear Models in Decision Making Research

In the empirical analyses which follow, I will evaluate the two theories of process incrementalism and of serial judgment primarily upon the basis of cross-sectional program or bureau level budgetary data which have been transformed into a percent change format. The goal of the modeling sections which follow will be to use the methodology of stochastic processes to derive competing probability distribution predictions. Since this approach and the methodology upon which it is based are unorthodox for budgetary decision research, a few words of justification are in order.

As Wanat (1974) and others have already argued, a linear decision rule interpretation of the Davis et al. regression results is open to dispute. Incrementalist regressions certainly demonstrate that federal budgetary decisions usually differ in only a marginal, temporally stable, and approximately linear manner from earlier decisions. Whether such descriptive patterns have actually been generated by the application of incrementalist decision rules is more problematic.

To appreciate the potential severity of this methodological issue, consider the following list of conceivable alternative decision processes:

1. an economic cost-benefit process in which the measurements of benefits and costs change in a marginal and stable manner over time (classical public finance approach);
2. an overt bargaining process in which non-incrementally motivated decision makers in conflict jointly determine budgetary allocations within a system in which "power relationships" remain approximately stable (political conflict of interest approach);
3. a decomposition process in which aggregate fiscal policy based totals, which themselves change only marginally, are sequentially suballocated into smaller and smaller sets of categories in an at least approximately proportional manner (Simon's "nearly decomposable systems" approach);
4. a selection process in which randomly generated proposals are rejected by hostile political environments which do not change radically in composition or selection criteria over time (evolutionary approach);
5. an exogenous determinants process in which "service levels" are set in response to clientele pressures which build and diminish relatively smoothly over time (municipal finance's public expenditure determinants approach); or even
6. a null model in which nothing but inflation drives the system.

All of these schematically described processes *could,* under plausible circumstances, generate allocational decisions which differed in only a marginal, temporally stable, and approximimately linear manner from earlier decisions. Of course, this is not to say that all of these processes *necessarily* lead to patterns of decisions which would be well tracked by the Davis et al. models, nor that one cannot imagine circumstances in which these processes would lead to differing predictions. However, the examples illustrate the point that process interpretation of incrementalist regression results is not straightforward.

I would suggest, however, not only that this problem of multiple process consistency with incrementalist empirical results is severe, but also that this problem is exacerbated by reliance on linear regression methodology. While to some extent such multiple process consistency problems are inevitable for any formal model, linear regression analysis of raw-dollar, time-series allocation data seems a peculiarly insensitive strategy for making "critical test" empirical distinctions among competing decision process theories.

My pessimism in this regard stems in part from the experience of the discipline of psychology with linear models of behavioral choice theories (Dawes and Corrigan, 1974). There, a number of simulations have been performed which show that a wide variety of nonlinear decision process models can generate final choices which are well tracked by simple linear regressions of those choices on information inputs. For example, Rorer (1971) has simulated graduate school admissions choices by ten different nonlinear decision rule models, ranging from quadratic models to multiplicative models to various sequential branching models to categorical or pattern recognition models to lexicographic models. Multiple regressions of simulated candidate ratings on information inputs, however, produced an average correlation coefficient of .85, with a maximum of .96 and a minimum of .71. Thus, even though the "true" underlying decision processes were radically nonlinear in character, the general linear model tracked aggregate patterns of final choices fairly well.

One can draw one of two conclusions from this type of result. On the one hand, linear functional forms are often quite useful for modeling and predicting final decision outcomes even when the underlying choice process is not understood. In the context of estimation, moreover, linear regression models are valuable (as long as prescribed precautions are taken against standard econometric problems such as heteroscedasticity, etc.) for investigating relative magnitudes of "net causal effects" of some independent variables on some dependent variable of interest. Such relative assessment of "net causal effects" can be made whether or not one understands the detailed mechanisms by which such effects are transmitted.

On the other hand, the very adaptability which makes linear models useful in these predictive and estimation contexts makes linear models indiscriminant in an explanatory context in which the goal is to understand the underlying process mechanisms by which final decision outcomes are produced. General linear models are all too robust in such a context for essentially two reasons. First, as Dawes and Corrigan (1974) demonstrate through simulation, linear models are quite robust in goodness-of-fit under deviations from optimal regression coefficient estimates. This appears to explain thg high R^2's obtained in Wanat's "uniformly distributed percent cuts" simulation exercise. Perhaps more importantly, however, behavioral decision data are very commonly characterized by conditionally monotone relationships between information inputs and choice outputs, for the simple reason that information is a cue for dimensions of value. These relationships insure that such data will to a first approximation have strong linear components, especially if the range of dependent variable variation is not great. In the face of competing process explanations, however, the demonstration of such linear components is frequently not very discriminating (Green, 1968). Instead of a futile search for marginal improvements to already high R^2's, therefore, I recommend a change in methodological focus—a reaffirmation of the "critical test" philosophy, in which the goal is to identify dimensions of data in which the predictions of competing theories differ markedly.

Stochastic Process Methodology

The alternative stochastic process approach to be employed in this article is designed to alleviate some of these methodological problems. The primary justification for a focus on predicted probability densities of cross-sectional percent allocation change is the fact that such predictions are clearly distinguishable and, hence, falsifiable relative to one another.

Focusing explicitly on the analysis of changes in allocations rather than on the analysis of absolute dollar levels is one straightforward first step towards the elimination of dominant but indiscriminant monotonic patterns from data. Models which predict change obviously predict absolute levels as well, but such models are more likely to diverge in statistical evaluation due to typically lowered goodness-of-fits. Standardization of changes in terms of percentages insures comparability of programs of various sizes within a cross-sectional grouping framework.

Cross-sectional grouping of programs at single points in time offers some promise for decision process research because it tends to bring into focus the object of study better than grouping across time for individual programs. Cross-sectional grouping of data across programs tends to hold constant sets of actors and organizational procedures, even as it increases the diversity of the program information cues being evaluated within the system. Time-series grouping, on the other hand, tends to hold constant the types of information cues being focused on, even as it increases variability in actors and organizational procedures. However, while cross-sectional budgetary analysis is arguably superior to time-series analysis for largely methodological reasons, simulation techniques also will be used in this article to compare the time-series behavior of serial judgment with process incrementalism.

Proper level of data aggregation is a complex issue for budgetary analysis, since quite different considerations may govern decision making at the various levels of overall budget total, departmental (e.g., HEW) allocations, and program (e.g., Vocational Rehabilitation) allocations. Indeed, as pointed out by Crecine (1975, 1977), in the executive branch higher-level fiscal policy outcomes may successively constrain less aggregate levels of budgetary decision making.

Formally, this hierarchical decision structure presents no problem, since it may be represented by a decomposable model in which the parameters of one level of analysis are taken as deriving in part from more aggregate considerations. In this article, however, parameters will be taken as exogenously fixed, and the focus of attention will remain on the program level of analysis for three reasons: (1) Theoretically, the primary concern here is with behavioral decision theory, which corresponds most naturally to the more micro level of budgetary decision making. (2) This program level corresponds to the Davis et al. bureau level of aggregation.[8] (3) This level of analysis mitigates "compositional effect" problems, which stem from the possibility that departmental totals reflect in aggregate the mixed outcomes of radically different program decision processes.

As stated above, the objective of the stochastic process technique is to derive probability density predictions for observable phenomena such as percent allocation change. The use of this technique will be illustrated in the following sections, but one introductory comment is in order. Stochastic process derivations of predicted program choice distributions can be carried out under two different assumptions—density parameters are homogeneous across programs, and density parameters are heterogeneous across programs. The heterogeneity assumption is clearly the more plausible of the two, since it permits decision makers to hold various substantive opinions about the merits of different programs. Heterogeneity, therefore, will be introduced into the following deviations via the technical device of postulating mixing distributions of parameters across programs. Both the homogeneous and the heterogeneous versions of the two process models, however, will be evaluated empirically.

The actual data to be analyzed here were drawn from the Crecine archive of internal Office of Management and Budget (OMB) planning documents and memoranda, and from the Padgett archive of internal Department of Housing and Urban Development (HUD) planning documents and memoranda. These executive branch data were supplemented with congressional data drawn from the Senate Appropriations Committee publication *Appropriations, Budget Estimates, Etc.*

Budget stages coded included the following:

CE_i = OMB's current estimate,[9] at the time of

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[8] Semantic ambiguities sometimes confuse aggregation descriptions in budgeting. For example, the term "agencies" refers in some instances to departments (e.g., HEW) and in others to bureaus (e.g., Bureau of Indian Affairs). Likewise, the term "programs" sometimes refers to entities very comparable to bureaus (e.g., HUD's Urban Renewal program, HEW's Family Services or AFDC program) and sometimes to subbureau entities, such as PPBS categories. In this article, the term "program" refers operationally to that set of sub-departmental units which appears in OMB internal planning tabulations. Typically, these units are equivalent to what Davis et al. refer to as "agencies" or bureaus. However, occasionally but consistently OMB will group small budget line items together into categories such as HEW's Community Health program(s). A listing of the "program allocation" data employed here can be found in Padgett (1978).

[9] The "Current Estimate" is the most recent OMB evaluation of the NOA and Expenditure implications of congressional appropriations, after taking into account supplementals and uncontrollable adjustments.

Preview ceilings, of the previous year's New Obligational Authority (NOA) for program *i;*

$\tilde{B}_i =$ OMB's spring or summer Preview NOA ceiling for program *i;*

$A_i =$ Domestic agencies' NOA fall submission or request to OMB for program *i;*

$\bar{B}_i =$ OMB's Director's Review NOA allowance for program *i;*

$P_i =$ Presidential NOA budget submission to Congress for program *i;*

$C_i =$ Congressional NOA appropriation for program *i.*

These raw-dollar budget data were then transformed into the following "percentage change" formats:

$$\Delta\tilde{b}_i = \frac{\tilde{B}_i - CE_i}{CE_i}\,;\ \Delta a_i = \frac{A_i - \tilde{B}_i}{\tilde{B}_i}\,;\ \Delta\bar{b}_i = \frac{\bar{B}_i - \tilde{B}_i}{\tilde{B}_i}\,;$$

$$\Delta p_i = \frac{P_i - \bar{B}_i}{\bar{B}_i}\,;\ \Delta c_i = \frac{C_i - P_i}{P_i}.$$

More complete institutional descriptions of the federal budgetary planning cycle can be found in Crecine (1977), LeLoup (1977), and Padgett (1978).

The Process Incrementalism Model

To operationalize the theory of process incrementalism within a stochastic process framework, we first have to specify linear "decision rules" for each of the above budget stages. This specification involves both a choice about the relevant "base" and a choice about appropriate incrementalist functional forms.

Here, the "base" will be taken in most cases to be simply the most recent prior decision. The only ambiguity in this operationalization comes in the case of the OMB allowance stage, where it is unclear whether the "base" should be the most recent decision made by agencies—agency requests—or the most recent decision made by OMB itself—preview ceilings. Here, the preview ceiling will be the allowance "base" under the assumption that this choice is more likely to be closer to OMB's allowance preferences than is the typically higher agency request.

The decision rule functional form to be examined will be the simple linear specification with one independent variable. This was the specification which Davis, Dempster and Wildavsky found to be empirically dominant. In fact, however, it will be seen that the distributional implications of incrementalist theory are independent of which particular linear "decision rule" is chosen for analysis.

Under these assumptions, the process incremental hypothesis that allocation decisions are produced "as if" they were generated by the application of simple and temporally stable linear decision rules may be operationalized as follows:

$$\tilde{B}_i = \alpha_{1i}\cdot CE_i + \epsilon_i$$
$$A_i = \alpha_{2i}\cdot \tilde{B}_i + \phi_i$$
$$\bar{B}_i = \alpha_{3i}\cdot \tilde{B}_i + \theta_i$$
$$P_i = \alpha_{4i}\cdot \bar{B}_i + \eta_i$$
and $$C_i = \alpha_{5i}\cdot P_i + \nu_i.$$

A simple transformation yields:

$$\Delta\tilde{b}_i = \alpha_{1i} - 1 + \epsilon_i/CE_i$$
$$\Delta a_i = \alpha_{2i} - 1 + \phi_i/\tilde{B}_i$$
$$\Delta\bar{b}_i = \alpha_{3i} - 1 + \theta_i/\tilde{B}_i$$
$$\Delta p_i = \alpha_{4i} - 1 + \eta_i/\bar{B}_i$$
and $$\Delta c_i = \alpha_{5i} - 1 + \nu_i/P_i.$$

Therefore, under incrementalist decision rule specifications, the problem of finding the predicted distribution of standardized change reduces to the problem of finding the distribution of decision rule parameters and the distribution of the standardized error terms. The latter of these two distributions is straightforward—since all previous decisions are fixed at the time of choice and hence are not random variables themselves, the Davis et al. hypothesis that stochastic disturbances are Normally distributed implies that the distributions of standardized stochastic disturbances are also Normal.

The main theoretical question, therefore, centers around the distribution of α_i's which is implicit in incrementalist theory. Davis, Dempster and Wildavsky said little in their original 1966 paper about the determination of such parameters, except to note that they should change only occasionally during "shift points." A later (1974) paper, however, presents a more explicit exogeneous determinants theory about how decision rule parameters are fixed during shift points. That theory may be labeled a "cumulative pressure" theory. Hence,

> While the incremental behavior specified by these models . . . appeared to be the general rule, a major finding concerned the nature of the exceptions. For many agencies, epochs in which the underlying relationships appeared to change were identified statistically. . . . We investigated a subset of these epochs by docu-

> mentary analysis and classified the major influences at work.... Although some of these influences were essentially random and nonrecurring, most could be seen to be due to specific political, or general economic or social events. This suggests that, although it is basically incremental, the budget process does respond to the needs of the economy and society, but only after sufficient pressure has built up to cause abrupt changes precipitated by these events (1974, p. 3).

Thus, the process image which Davis et al. present regarding "shift point" changes in decision rules is one in which a variety of exogeneous events influencing "the needs of the economy and society" exert pressure on the internal operation of the budgetary process. This pressure in turn cumulates, without immediate effect, until decision makers recognize or are forced to recognize the need for change, at which point an abrupt transformation of decision rules occurs which reflects the aggregate level of all of this cumulated pressure. This budgetary theory is consistent with more general standard operating procedure theory (Cyert and March, 1963; Steinbruner, 1974), which presumes that simple decision heuristics shift abruptly only when a sufficiently severe "problem" forces the updating and revision of past learning.

Davis, Dempster and Wildavsky (1974, p. 9) operationalize this verbal image of cumulated pressure caused by exogenous events in the following manner:

$$\alpha_i = \beta_{0i} + \beta_{1i}z_1 + \beta_{2i}z_2 + \ldots + \beta_{mi}z_m.$$

The z's here represent all those political, economic and social events or variables which determine the perceived "need" for a program and which, hence, put pressure on the budgetary process regarding that program. Davis et al. actually studied 18 such variables, but they leave little doubt that an even larger number of variables are likely to be operative. The joint effect of excluded variables was grouped into Normal regression "error" terms.

This cumulative pressure theory of "shift point" changes, therefore, states that (a) there exists a wide variety of possible exogenous event determinants of the α_i's, and that (b) these determinants are cumulative or additive in their effects.

Given these two substantive hypotheses, one of two approaches can be taken to generate the desired predicted distribution of α_i. Both, however, lead to the same answer. If one presumes the z's to be nonstochastic, as is implicit in the regression framework which Davis, Dempster and Wildavsky themselves adopt, then the distribution of α_i is identical to the distribution of the regression stochastic disturbance term, which of course is Normal. If, on the other hand, one presumes the z's to be stochastic, then, under the supplementary assumption that the z's are independent, the Central Limit Theorem implies that α_i again approaches Normality, regardless of the distribution of the z's.

Two comments can be made about the supplementary independence assumption required to make the Central Limit Theorem argument hold in the stochastic z approach. First, Davis et al. themselves implicitly adopt this very common methodological assumption in their stochastic disturbance representation of excluded variables. More importantly, however, advanced versions of the Central Limit Theorem (Loève, 1955, p. 377) have shown that even sums of dependent random variables converge to Normals under certain technical conditions.[10] In less precise terms, these advanced results imply that the sum of even dependent random variables will converge to a Normal distribution as long as one one summand is "dominant" in the presence of an infinite number of other summands. This fact appears to account for the robustness of Normal approximations of sums even when common independence assumptions are violated.

Hence, under at least two different interpretations, the Davis, Dempster and Wildavsky "shift point" theory implies that all of their decision rule parameters should be at least approximately Normally distributed. For example,

$$p(\alpha_{1i}) = \Phi[\mu_{1i}, \sigma^2_{1i}].$$

This result in turn, coupled with the linear decision rule specification listed above, implies that the density of standardized allocation choices predicted by process incrementalism should also be Normal, since the convolution of any number of Normals remains Normal. For example,

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[10] These technical conditions are (a) that the expected differences between finite conditional and finite unconditional means and variances of z_m's approach zero as $m \to \infty$, and (b) that the so-called Lindeberg-Feller condition holds. This last condition implies that the ratio of any z's variance to the variance of the sum approaches zero as $m \to \infty$ (Woodroofe, 1975, p. 256).

$$p(\Delta\tilde{b}_i) = \Phi[\mu'_{1i}, \sigma'^{2}_{1i}],$$

where

$$\mu'_{1i} = [\mu_{1i} - 1 + (\mu_{\epsilon_i}/CE_i)],$$

and

$$\sigma'^{2}_{1i} = [\sigma^2_{1i} + (\sigma_{\epsilon_i}/CE_i)^2].$$

This convolution property accounts for the earlier remark that distributional implications of process incrementalism are independent of the particular linear decision rule specification. Increasing the number of terms in a linear decision rule simply increases the number of Normally distributed standardized parameters terms which must be convoluted. But such an increase in no way affects the final distributional form.

This distributional result holds for individual programs. That is, process incrementalism implies that the probability density of allocation choice for any program at a fixed point in time is Normal. This individual program prediction, however, will be identical to the prediction that observable cross-program distributions are Normal only under the supplementary assumption that all program density parameters are constant across programs. In fact, however, a heterogeneity assumption, reflecting different information inputs and substantive policy priorities, is more plausible.

Incorporating heterogeneity into the analysis requires the postulation of mixing distributions of parameters across programs. Particular such hypotheses are hard to justify on theoretical grounds, so I will simply adopt the strategy of presuming mixing distributions which are tractable but which also are sufficiently malleable to at least approximate virtually any unimodal reality. The only constraints in this Normal case, of course, are that μ parameters lie between $-\infty$ and $+\infty$ and that σ^2 parameters lie between 0 and $+\infty$.

Two mixing distributions which appear reasonable under this strategy are the following: (a) The distribution of means across programs is Normal $(\bar{\mu}, \bar{\sigma}^2)$. (b) The distribution of the inverse of variances across programs is Gamma (α, β). The first of these two heterogeneity assumptions is straightforward in the absence of any theoretical reasons to the contrary. The second of these two assumptions is convenient since Gammas are sufficiently malleable under different (α, β) selections to at least approximate most unimodal patterns.[11]

The implications of these two heterogeneity assumptions, taken one at a time for the illustrative case of $\Delta\tilde{b}_i$, are as follows: (a) A Normal distribution of μ_{1i}'s across programs, with σ^2_{1i}'s temporarily held constant, implies the following Normal distribution of $\Delta\tilde{b}_i$'s:

$$p(\Delta\tilde{b}_i) = \Phi[\bar{\mu}, \sigma^{*2}_i],$$

where

$$\sigma^{*2}_i = [\bar{\sigma}^2 + \sigma^2_{1i} + (\sigma_{\epsilon_i}/CE_i)^2].$$

(b) However, if $(\sigma^{*2}_i)^{-1}$ in turn is assumed to be distributed as Gamma (α, β) across programs, then the predicted distribution of percent change program choices becomes a type of Student's *t* distribution:

$$p(\Delta\tilde{b}_i) = \frac{(2\beta)^{-1/2}}{B(1/2,\alpha)} \left[1 + \frac{(\Delta\tilde{b}_i - \bar{\mu})^2}{2\beta}\right]^{-(\alpha+1/2)},$$

where $B(1/2, \alpha)$ is the symbol for the beta function.

Thus under an assumption of parameter homogeneity, process incrementalism implies a Normal distribution of appropriately standardized allocation change. However, under the more plausible assumption of parameter heterogeneity, process incrementalism implies approximately a Student's *t* distribution of standardized allocation change. Not surprisingly, these two distributions are not qualitatively very different from one another, the main difference being the slightly "fatter tails" of the Student's *t*.

The Serial Judgment Model

To operationalize the competing serial judgment theory of budgetary decision making, first posit that the historical "starting points" required for each budgetary stage are identical to the "bases" specified for the process incrementalism model. Given these fixed historical referents, however, the serial judgment decision maker then decides upon a direction of search —namely, upon whether some increase or some decrease is warranted both "on the merits" and

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[11] For example, Gammas can assume a J pattern with peak at 0, or unimodal patterns with peak anywhere from 0 to ∞. Moreover, this class subsumes within it the Chi-Square and Negative Exponential distributions as special cases.

"under the current fiscal climate."

This first "direction of search" phase can be represented, for the illustrative case of OMB ceilings, as a binary choice between the two alternative sets $\Delta\tilde{b}_i > 0$ and $\Delta\tilde{b}_i < 0$. This binary choice is based upon the following two judgmental propensities:

p_i = the probability of deciding that some allocation increase for program *i* is justified "on the merits," and

$\bar{p}$ = the probability of deciding that an allocation increase for any program is justified "under the current fiscal climate."

Assuming that these two judgments are independent, the "direction of search" choice can be modeled as

$$P[\Delta\tilde{b}_i > 0] = \bar{p} \cdot p_i$$
$$P[\Delta\tilde{b}_i < 0] = 1 - \bar{p} \cdot p_i.$$

Given search, the serial judgment decision maker next perceives discrete salient alternatives in an essentially "unbiased" or random manner over the continuous space of all conceivable allocation alternatives. More specifically, hypothesize that information is such that the probability of perceiving a salient alternative in any small interval of allocation dollars (or percentages) is constant over all possible small intervals, be they located among large or among small allocation alternatives. In other words, again for the illustrative case of OMB ceilings:

I. The probability of perceiving at least one salient alternative in the small allocation interval $d\Delta b_i$ is

$$P(d\Delta\tilde{b}_i) = \lambda_i d\Delta\tilde{b}_i + o(d\Delta\tilde{b}_i);\ d\Delta\tilde{b}_i \to 0, \lambda_i > 0.$$

II. The probability of perceiving two or more salient alternatives in the interval $d\Delta\tilde{b}_i$ is $o(d\Delta\tilde{b}_i)$, where $o(d\Delta\tilde{b}_i)$ is a technical term which can be interpreted as "negligible probability."

It is shown in many probability tests (e.g., Feller, 1968, pp. 446–48), that these two hypotheses are sufficient to imply that the resultant full distribution of perceived salient alternatives over the large interval $[0, \Delta\tilde{b}_i)$ is Poisson. That is,

$$P[X_i = k] = \frac{\lambda_i^k}{k!} e^{-\lambda_i \Delta\tilde{b}_i}.$$

Here, X_i is the random variable "number of salient alternatives perceived in the interval $[0, \Delta\tilde{b}_i)$," and k represents particular integer sample outcomes of X_i. λ_i is a parameter which equals the expected value of X_i. This measure of how "fine-grained" decision makers' perceptions of alternatives are is inversely correlated with the program's "degree of controllability."[12]

For purposes of parsimony, the simplifying assumption will be adopted that this same λ_i is also applicable for the case of $\Delta\tilde{b}_i < 0$. This symmetry simplification, however, will be reexamined in light of later data analysis.

Choice under the serial judgment decision strategy is simply a matter of cycling through the salient alternatives encountered along the direction of search until one of them is deemed acceptable both on the merits and under the current fiscal climate. Let

β_i = the probability of deciding that a salient alternative is acceptable, based "on the merits," and

$\bar{\beta}$ = the probability of deciding that a salient alternative is acceptable "under the current fiscal climate."

Then, the probability of accepting a salient alternative as adequate is $\bar{\beta}\cdot\beta_i$, and the probability of rejecting a salient alternative as inadequate is $1 - \bar{\beta}\cdot\beta_i$. Under this specification, $1 - \bar{\beta}\cdot\beta_i$ may be thought of as a stochastic analogue to Simon's more deterministic notion of "aspiration level," since probability of rejection increases the higher the aspiration level.

To derive the predicted density of percent change allocation choice from this model, all that is necessary is to notice that, for a fixed direction of search, final choice will be the sum of all of the salient alternatives added to the base. In other words,

$$\Delta\tilde{b}_i = \sum_{j=0}^{n} \Delta\tilde{b}_{ij},$$

where the $\Delta\tilde{b}_{ij}$'s represent the successive sizes of salient alternatives considered, and n represents the total number of alternatives accepted. Both $\Delta\tilde{b}_{ij}$ and n are random variables. Hence, the desired $p(\Delta\tilde{b}_i)$ is a compound distribution of $p(\Delta\tilde{b}_{ij})$ and $p(n)$.

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[12] That is, the more controllable the program, the larger, on average, discrete dollar or percent alternatives are likely to be. Moreover, the larger the average size of individual alternatives, the fewer of them are likely to be perceived within any fixed interval.

The distribution $p(\Delta\tilde{b}_{ij})$ follows from the above Poisson "unbiased" perception of salient alternatives hypothesis. For it can be shown (Karlin and Taylor, 1975, p. 121) that the distribution of distances between Poisson events is Exponential (λ_i), and n-fold convolutions of such Exponentials are Gamma (n, λ_i). Moreover, it can be shown (Feller, 1968, pp. 164–68) that the distribution of number of evaluations until the first acceptance "stopping point" is Geometric ($\bar{\beta}\cdot\beta_i$). Therefore, fixing the direction of search to be positive, and using the notation $p(\Delta\tilde{b}_{ij})^{n*}$ to represent the n-fold convolution of $p(\Delta\tilde{b}_{ij})$ with itself, the serial judgment prediction for the distribution of percent allocation change is Exponential $[\lambda_i(1-\bar{\beta}\beta_i)]$ with a singularity at $\Delta\tilde{b}_i=0$, as is demonstrated in Figure 1.

Coupling this result with first "direction of search" stage is straightforward:

$$p(\Delta\tilde{b}_i)=\begin{cases}(1-p_i^*)\beta_i^*\lambda_i^*e^{\lambda_i^*\Delta\tilde{b}_i}, & \Delta\tilde{b}_i<0\\ (1-\beta_i^*) \quad , & \Delta\tilde{b}_i=0\\ p_i^*\beta_i^*\lambda_i^*e^{-\lambda_i^*\Delta\tilde{b}_i} \quad , & \Delta b_i>0,\end{cases}$$

where $p_i^*\equiv\bar{p}p_i$, $\beta_i^*\equiv\bar{\beta}\beta_i$ and $\lambda_i^*\equiv\lambda_i(1-\bar{\beta}\beta_i)$. This Double Exponential prediction is analogous to the process incrementalism prediction of a Normal Density of allocation choice for any one individual program.

As was the case in the last section, however, I also wish to explore the implications of the plausible assumption that parameters are heterogeneous across programs. Using the same strategy of exploring mixing distributions which are both tractable and malleable, the following hypotheses seem reasonable, given that $\bar{p}p_i$ and $\bar{\beta}\beta_i$ are constrained to lie between 0 and 1 and that λ_i is constrained to lie between 0 and $+\infty$:

(a) p_i^* is distributed as Beta (u,v).[13]

(b) β_i^* is distributed as Beta (υ,ν).

(c) λ_i is distributed as Gamma (α,δ).

Under these heterogeneity assumptions, with the added constraint that $\alpha=\upsilon+\nu$, it can be shown that the cross-program empirical distribution of choices produced by a serial judgment maker will be a Double Pareto:

$$p(\Delta\tilde{b}_i)=\begin{cases}(1-u_1^*)u_2^*(\frac{\nu}{\delta})\,[1-(\frac{1}{\delta})\Delta\tilde{b}_i]^{-(\nu+1)}, & \\ & \Delta\tilde{b}_i<0\\ (1-u_2^*), & \Delta\tilde{b}_i=0\\ u_1^*u_2^*(\frac{\nu}{\delta})[1+(\frac{1}{\delta})\Delta\tilde{b}_i]^{-(\nu+1)}, & \\ & \Delta\tilde{b}_i>0.\end{cases}$$

In this case $u_1^*\equiv\dfrac{\mathrm{u}}{\mathrm{u+v}}$ and $u_2^*\equiv\dfrac{\upsilon}{\upsilon+\nu}$.

Thus, the serial judgment process produces a Double Exponential empirical distribution of

$$p(\Delta\tilde{b}_i)=\sum_{n=0}^{\infty}p(n)p(\Delta\tilde{b}_{ij}\,|\,n)$$

$$=p(n=0)p(\Delta\tilde{b}_{ij}\,|\,n=0)+\sum_{n=1}^{\infty}p(n)p(\Delta\tilde{b}_{ij})^{n*}$$

$$=(1-\bar{\beta}\beta_i)\begin{Bmatrix}1, \Delta\tilde{b}_i=0\\ 0, \Delta\tilde{b}_i>0\end{Bmatrix}+\sum_{n=1}^{\infty}(1-\bar{\beta}\beta_i)\,(\bar{\beta}\beta_i)^n\begin{Bmatrix}0 & , \Delta\tilde{b}_i=0\\ \dfrac{\lambda_i^n}{(n-1)!}(\Delta\tilde{b}_i)^{n-1}e^{-\lambda_i\Delta\tilde{b}_i} & , \Delta\tilde{b}_i>0\end{Bmatrix}$$

$$=\begin{cases}(1-\bar{\beta}\beta_i) & , \Delta\tilde{b}_i=0\\ (\bar{\beta}\beta_i)\lambda_i(1-\bar{\beta}\beta_i)e^{-\lambda_i(1-\bar{\beta}\beta_i)\Delta\tilde{b}_i} & , \Delta\tilde{b}_i>0\end{cases}$$

Source: Compiled by the author.

Figure 1. Serial Judgment Derivation for the Individual Program Level

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[13] The Beta distribution is very malleable since, under various selections of u and v parameters, this distribution can assume J shapes with peak at either 0 or 1, U shapes with peaks at 0 and 1, unimodal shapes, triangular shapes, or even the rectangular shape of the Uniform distribution.

percent change allocation choices for the case of homogeneous programs. And it produces a Double Pareto empirical distribution of percent change allocation choices for the more plausible case of heterogeneous programs.

Graphically, the competing distributional predictions of process incrementalism and serial judgment are illustrated in Figure 2.

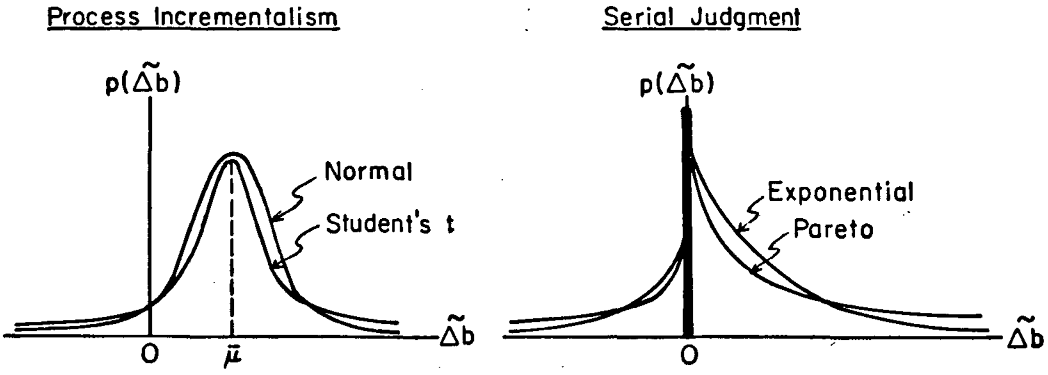
Substantively, the characteristic sharp peak near zero of the Double Pareto implies that most programs most of the time receive budget allocations which are only marginally different from the historical referent or base. The equally characteristic "fat tails" of the Double Pareto, however, imply the occasional occurrence of very radical changes. What Davis, Dempster and Wildavsky describe as a "shift point," induced by exogenous intervention into a routinized budget system, the serial judgment strategy generates as the normal outcome of its more flexible decision process.

Data Analysis

As mentioned above, the program allocation data to be analyzed here were drawn from the Crecine OMB and the Padgett HUD archives, and were supplemented with publically available congressional data. The years chosen for analysis were fiscal years 1957, 1964, and 1966. These three particular years were selected within data availability constraints to represent one year each from the Eisenhower, the Kennedy, and the Johnson administrations. Only domestic program data were coded because of the Crecine (1975) finding that allocational decision making for National Security programs is relatively autonomous, institutionally, from decision making for domestic programs. Also, very small programs, whose Preview Current Estimate totalled less than $1 million, were excluded from the analysis. In all, complete data series for 62, 98 and 94 domestic programs could be reconstructed for FYs 1957, 1964, and 1966, respectively. These programs represented 83, 91, and 86 percent of the total NOA domestic[14] President's budget for FYs 1957, 1964, and 1966. A more complete description and listing of the program allocation data analyzed can be found elsewhere (Padgett, 1978).

In particular, the "major budget stages" coded are listed in Table 1. More than the usual number of stages were coded for FY 1957 because of the greater emphasis placed on these early planning figures by the Eisenhower administration. An additional FY 1964 ceiling revision stage was coded because of Kennedy's unanticipated midyear decision to cut the budget total by $2 billion in order to increase the chances of congressional passage of the first major Keynesian tax-cut measure.

As has already been discussed, the data were first standardized into a percent change format. Besides the $\Delta\tilde{b}$, Δa, $\Delta\bar{b}$, Δp, and Δc transformations already mentioned, the standardizations employed were the following:



Source: Compiled by the author.

Figure 2. Graphical Display of Incrementalism versus Serial Judgment Distributional Predictions

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[14] The term "domestic" is meant to exclude the Department of Defense-Military, the Atomic Energy Commission, the National Aeronautics and Space Administration, and Foreign Military Assistance.

$$\Delta\tilde{b}_p = (\frac{\text{1957 Projection} - \text{1956 Current Estimate)}}{\text{1956 Current Estimate}})$$

$$\Delta a_p = (\frac{\text{1957 Preview Request} - \text{1957 Projection}}{\text{1957 Projection}})$$

$$\Delta\tilde{b}' = (\frac{\text{1957 Ceiling} - \text{1957 Projection}}{\text{1957 Projection}})$$

$$\Delta\tilde{b}_r = (\frac{\text{1964 ``Ratcheted'' Ceiling} - \text{1964 Ceiling}}{\text{1964 Ceiling}}).$$

The primary statistical procedure employed for the relative evaluation of the process incrementalism and the serial judgment theories was the Kolmogorov-Smirnov one-sample test. The Kolmogorov-Smirnov statistic is the maximum deviation over all cases of the empirical cumulative distribution from the predicted cumulative probability distribution (Bickel and Doksum, 1977). Hence, the lower the statistic, the better the goodness-of-fit.

One problem with the direct calculation of Kolmogorov-Smirnov statistics in the present research context is the fact that the full serial judgment models, as developed above, possess one extra free parameter than do the corresponding process incrementalism models. This problem was solved by constraining Exponential β_i^* and Pareto u_2^* parameters to equal 1.0. With or without this comparability constraint, however, all relevant density parameters were estimated by the method of maximum likelihood, except for Student's *t* parameters which were estimated by the method of moments.[15]

Table 2 reports finally derived Kolmogorov-Smirnov goodness-of-fit statistics, along with critical values for the .05 level of significance. Results for both constrained and unconstrained versions of the serial judgment model are given. The critical values reported are the Monte Carlo values cited by Bickel and Doksum (1977, p. 381), which are exact for the Normal case with parameters estimated and which are approximations otherwise.[16]

The results are remarkably consistent over all years and over all budget stages. In 17 out of the 18 budget stages analyzed, serial judgment predictions are superior to process incrementalism predictions when numbers of parameters are constrained to be comparable. Moreover, even this one discrepant $\Delta\tilde{b}_r$ stage vanishes when the unconstrained serial judgment model is estimated, since almost half of the program cases in this stage were observed to equal zero percent change. When the full serial judgment model is estimated, all Double Pareto statistics drop below the critical values cited. It is shown elsewhere (Padgett, 1978) that these general comparative findings are not altered if one employs average absolute error, sum of squared errors, or χ^2 as alternative goodness-of-fit measures.

Thus on the basis of more refined cross-sectional analysis of percentage allocation change, the theory of serial judgment appears more consistent with program-level federal budgetary decision making than does the Davis, Dempster and Wildavsky theory of process incrementalism. It can be shown, moreover, that this superiority is not an artifact of uncontrollability in the budget process (Padgett, 1978).

One oversimplification of the serial judg-

Table 1. Budget Stages Coded

FY 1957	FY 1964	FY 1966
OMB Preview Current Estimates	OMB Preview Current Estimates	OMB Preview Current Estimates
OMB May Projections	OMB Preview Ceilings	OMB Preview Ceilings
Agency Preview Requests	OMB "Ratcheted" Ceilings	Agency Requests
OMB Preview Ceilings	Agency Requests	OMB Director's Review Allowances
Agency Regular Requests	OMB Director's Review Allowances	President's Budget
OMB Director's Review Allowances	President's Budget	Congressional Appropriations
President's Budget	Congressional Appropriations	
Congressional Appropriations		

Source: Compiled by the author.

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[15] Maximum likelihood estimation for the Normal, Exponential and Pareto distributions is described in Johnson and Kotz (1970, Vol. 1). The method of moments (Johnson and Kotz, 1970, Vol. 2, p. 116) was employed for the Student's *t* due to convergence difficulties experienced in the numerical analysis of the three relevant log likelihood derivative functions.

[16] The standard Kolmogorov-Smirnov critical values reported in many statistics text tables are not appropriate here due to parameter estimation. Unfortunately, relevant Monte Carlo simulations for non-Normal cases could not be found in the literature.

ment model, however, should be noted. It is apparent from visual inspection of Double Pareto residuals that, over the range of typically less common decrease choices, Double Pareto predictions for agency requests are higher than observed cumulative distributions, and that, over the same range, Double Pareto predictions for OMB ceilings and allowances are lower than observed distributions. This suggests that the simplifying assumption that λ_i^* parameters are symmetric for both increase and decrease directions of search is somewhat in error. Apparently, once agencies decide that some decrease from ceiling is necessary, they are much more reluctant to grant sizable decreases than they are to grant comparable-size increases. On the other hand, once OMB decides that some decrease for a program is warranted, OMB appears much more eager to make such a decrease sizable than it does to grant comparable increases. These two observed biases, of course, are quite consistent with our substantive preconceptions about agencies' and OMB's respective roles in the budgetary process.

Despite the observed superiority of serial judgment's distributional predictions over process incrementalism's distributional predictions, it is of course always possible that the Double Pareto distribution could also be consistent with some other decision process theory not yet examined.[17] While no definitive answer to this problem exists, some suggestive information can be generated by examining the more "fine-grained" predictions of serial judgment

Table 2. Kolmogorov-Smirnov Statistics

		Serial Judgment				Incrementalism	
		(Unconstrained)		(Constrained)			
		Double Expon.	Double Pareto	Double Expon.	Double Pareto	Normal	Student's *t*
1957 (*n*=62)							
OMB Projections	$(\Delta\tilde{b}_p)$	.1938	.1029	.2104	.1003	.2366	.2307
Preview Requests	(Δa_p)	.1572	.0547	.1891	.0596	.3148	.3020
OMB Ceilings	$(\Delta b')$	.1460	.0544	.1510	.0691	.2310	.2292
Regular Requests	$(\Delta\underline{a})$	.1502	.0474	.1530	.0441	.2821	.2703
OMB D.R. Allowances	$(\Delta\bar{b})$	.1147	.0597	.1174	.0637	.2448	.2115
President's Budget	(Δp)	.1342	.0723	.2674	.2452	.4329	.4271
Congressional Approp.	(Δc)	.0936	.0528	.2198	.1850	.2513	.2572
Critical K-S value at p=.05				.1123			
1964 (*n*=98)							
Regular OMB Ceilings	$(\Delta\underline{\tilde{b}})$	.0794	.0611	.0812	.0547	.1666	.1472
Ratcheted Ceilings	$(\Delta\tilde{b}_r)$	.0566	.0564	.4490	.4490	.2539	.2733
Agency Requests	$(\Delta\underline{a})$	.1439	.0682	.1547	.0711	.2571	.2422
OMB D.R. Allowances	$(\Delta\bar{b})$	.1010	.0559	.1134	.0575	.2398	.2267
President's Budget	(Δp)	.1638	.0667	.2037	.1392	.3111	.2817
Congressional Approp.	(Δc)	.1209	.0671	.1473	.0767	.2574	.2210
Critical K-S value at p=.05				.0897			
1966 (*n*=94)							
OMB Ceilings	$(\Delta\tilde{b})$	.1900	.0787	.1945	.0745	.2771	.2680
Agency Requests	$(\Delta\underline{a})$	.1535	.0772	.1725	.0867	.2781	.2596
OMB D.R. Allowances	$(\Delta\bar{b})$	.1118	.0366	.1368	.0515	.2271	.2128
President's Budget	(Δp)	.1972	.0472	.2327	.0642	.3414	.3297
Congressional Approp.	(Δc)	.1783	.0514	.2170	.0785	.3484	.3410
Critical K-S value at p=.05				.0916			

Source: Data coded from Crecine OMB archive and Padgett HUD archive of U.S. government internal memoranda and budgetary planning documents.

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[17] In fact, I report elsewhere (1978) that the serial judgment model is also superior in its distributional predictions to two other stochastic models of budgetary decision making—a tacit bargaining model, which is a member of the rational choice tradition, and an information bombardment model, which is a member of the recent "organized anarchy" tradition of Cohen, March, and Olsen (1972). The fact remains, however, that possible alternative process consistency is a serious issue.

theory. Perhaps one of the most critical of such predictions is that observed Double Pareto distributions in fact derive from a mixture of Double Exponential distributions. This expectation suggests that if one could somehow group programs, on an a priori basis, into relatively more homogeneous parameter subsets, the observed distributions should be close to the more fundamental Double Exponential.

One obvious such grouping procedure is to subset programs according to their departmental memberships. It is at least plausible to speculate that, because of organizational structure, strategic or other reasons, program parameters are more homogeneous within departments than they are across departments. If this were the case, then the distributions of allocation choices for only HUD or HEW programs, for example, could be examined separately, via the Kolomogorov-Smirnov test or some other procedure, to see if such distributions are consistent with Double Exponential predictions.

Unfortunately, for the data analyzed here, this straightforward procedure is unreliable due to the small sample sizes of programs within departments. (The data set used here had an average of only eight programs within departments.) Under this situation, therefore, a χ^2 significance test of the more complicated compound hypothesis that all departments' distributions are Exponential simultaneously was constructed along the lines suggested by Bickel and Doksum (1977, p. 320). The procedure employed was the following: Using absolute value choices, individual department λ_i^* parameters first were estimated using the Newton iterative procedure suggested by Bickel and Doksum.[18] The χ^2statistic was then calculated for each budget stage over all departments on the basis of the four fixed-interval groupings $[.00, .05)$, $[.05, .10)$, $[.10, .20)$ and $[.20, \infty)$.

The results are reported in Table 3, along with the critical χ^2 $(p=.05)$ appropriate for m-s-1 degrees of freedom. (s here is the number of parameters estimated, equal to the number of departments in each stage; and $m=4\cdot s$ is the total number of intervals over which the χ^2 statistics were calculated.) As can be seen, the compound hypothesis that all departments' distributions are Exponential cannot be rejected for any budget stage. These results give some support to the serial judgment "fine-grained" expectation that observed Double Pareto distributions actually derive from a heterogeneous mixture of underlying Double Exponentials.

Table 3. χ^2 Test of Compound Hypothesis that All Individual Departments' Distributions are Exponential

1957 (n=53)		
OMB Projections	$(\Delta\tilde{b}_p)$	19.144
Preview Requests	(Δa_p)	23.898
OMB Ceilings	$(\Delta\tilde{b}')$	25.723
Regular Requests	(Δa)	13.289
OMB D.R. Allowances	(Δb)	16.724
President's Budget	(Δp)	16.649
Congr. Appropriations	(Δc)	9.024
Critical χ^2_{23} at p=.05		35.172
1964 (n=93)		
OMB Regular Ceilings	$(\Delta\tilde{b})$	30.866
Ratcheted Ceilings	$(\Delta\tilde{b}_r)$	38.984
Agency Requests	$(\Delta \underline{a})$	21.221
OMB D.R. Allowances	$(\Delta\bar{b})$	35.873
President's Budget	(Δp)	27.136
Congr. Appropriations	(Δc)	26.569
Critical χ^2_{35} at p=.05		49.802
1966 (n=91)		
OMB Ceilings	$(\Delta\tilde{b})$	40.790
Agency Requests	$(\Delta \underline{a})$	28.513
OMB D.R. Allowances	$(\Delta\bar{b})$	21.663
President's Budget	(Δp)	31.199
Congr. Appropriations	(Δc)	26.511
Critical χ^2_{35} at p=.05		49.802

Source: Data coded from Crecine OMB archive and Padgett HUD archive of U.S. government internal memoranda and budgetary planning documents.

One final empirical point is worthy of note. The consistency of serial judgment theory with the high regression R^2's reported by Davis et al. is easily demonstrated. Monte Carlo simulations of a sequence of Double Exponential choices have been performed, and incrementalist "decision rule" regressions have been run on the reconstructed time series dollar outcomes. For purposes of illustration, 56 program allocation "cases" were generated over 17 years[19] using p_i^*, β_i^* and λ_i^* parameters which were randomly selected from Beta and Gamma mixing distributions. The parameters of these mixing distributions, in turn, were taken to equal the average, over the three years studied, of the unconstrained parameters estimated for each $\Delta\tilde{b}$, $\Delta\bar{b}$,

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[18] β_i^* was constrained to equal 1, and departments with only three programs or less were excluded from the analysis. I am grateful to Mark Kamlet for bringing this procedure to my attention.

[19] These were the number of bureaus and years studied by Davis et al. (1966).

Δp, and Δc budget stage. Dollar time-series data were reconstructed from simulated percent change choices by means of the identities

$$\text{PRES. BUDGET}_{i,t} = \text{CONG. APPROP.}_{i,t-1}(1+\Delta\tilde{b}_{i,t})(1+\Delta\bar{b}_{i,t})(1+\Delta p_{i,t}),$$

$$\text{CONG. APPROP.}_{i,t} = \text{PRES. BUDGET}_{i,t}(1+\Delta c_{i,t}),$$

where the Preview Current Estimate here is taken to equal the simulated congressional appropriation. The incrementalist regressions run were of the simple form listed in the beginning of this article, without using any "shift points."

The resulting simulated distributions of *R*'s are reported in Table 4, along with the analogous distributions reported by Davis, Dempster and Wildavsky (1966, p. 537). It is apparent that the distribution of serial judgment simulation *R*'s is not substantially different from the distribution of actually estimated *R*'s. Hence, serial judgment theory is consistent with gross time-series patterns of federal budgetary data, as well as with more refined cross-sectional patterns. However, these simulation results also illustrate concretely the methodological argument presented above about the insensitivity of regression analysis in process-oriented budgetary decision-making research.

Conclusions

This article has examined the methodological foundations of incrementalist quantitative research and has proposed a new bounded rationality theory of budgetary decision making. The empirical results support the relative superiority of this serial judgment theory over the competing Davis, Dempster and Wildavsky theory of process incrementalism.

In some respects, these results support both the incrementalists and their critics. Certainly, the importance of the bounded rationality paradigm's emphasis on cognitive constraints and simplifying decision heuristics, which the incrementalists share, has been reaffirmed. In addition, the incrementalist notion of "base" has been given support in the context of serial judgment's reinterpretation of that concept as a "reference point." However, when the focus shifts from absolute dollar levels to the analysis of allocational change, the rigidity implicit in the incrementalist temporally fixed decision rule formulation is undercut. In its stead, serial judgment theory substitutes a systematic, yet very contextually dependent process of search through and informal evaluation of a limited number of salient alternatives.

This serial judgment process of decision making generates budgetary outcomes in which most program allocations most of the time differ in only a marginal, but temporally variable, manner from the historical base. However, occasionally, as the normal outcome of serial judgment decision making, more radical and "catastrophic" changes are also produced. Serial judgment theory implies that the federal budgetary system is much more responsive to political, bureaucratic and technical dynamics, on a routine even if constrained basis, than the theory of process incrementalism would lead one to believe.

Somewhat paradoxically, this formal serial judgment theory is quite consistent with Wildavsky's verbal description of *The Politics of*

Table 4. Frequencies of Regression Correlation Coefficients

A. Serial Judgment Simulation *R*'s (n=56)

	1 – .995	.995 – .99	.99 – .98	.98 – .97	.97 – .96	.96 – .95	.95 – .94	.94 – .93	.93 – .90	.90 – .85	.85 – 0
Executive	3	5	8	6	4	5	3	4	6	3	9
Congressional	28	8	7	1	2	2	2	1	3	1	1

B. Davis, Dempster and Wildavsky Reported *R*'s (Exec. n=61; Cong. n=67)

	1 – .995	.995 – .99	.99 – .98	.98 – .97	.97 – .96	.96 – .95	.95 – .94	.94 – .93	.93 – .90	.90 – .85	.85 – 0
Executive	9	2	2	8	5	2	4	3	5	11	10
Congressional	21	8	15	4	5	2	2	1	5	2	2

Source: Compiled by the author; Otto A. Davis, M. A. H. Dempster, and Aaron Wildavsky (1966), "A Theory of the Budgetary Process," *American Political Science Review* 60: 529–47.

the Budgetary Process. Certainly, Wildavsky's generalizations that "budgeting is experiential; . . . budgeting is simplified; . . . budgeting officials 'satisfice'; . . . budgeting is [frequently outcome] incremental" are embedded in the serial judgment formalization. Also the numerous strategic cues itemized by Wildavsky may be important components of the contextual information detail which lies behind serial judgment parameters. Indeed, one implication of the serial judgment model is that Wildavsky's original highly contextual and political emphasis need not be abandoned in the search for bounded rationality process regularities.

Future research should be directed toward an analysis of the actual substantive issue content which flows through this serial judgment process. In model terms, this suggests an investigation into the cross-program structure of estimated serial judgment parameters. One such extension which seems promising is to model serial judgment parameters in terms of the hierarchical organizational structure emphasized by Crecine. Indeed, it can be shown that formally embedding the serial judgment program-level process into a departmental-level attention-focusing process, in which salient alternatives are sequentially allocated to various programs according to a stationary probability "strategy vector" until a fixed aggregate fiscal target is reached, is also consistent with the empirically confirmed Double Pareto distribution (Padgett, 1979). This type of extension might be one approach to reconciling bounded rationality theories, such as serial judgment, with more aggregate organization theory and with historically oriented studies of budgetary management strategies.

Work in this direction is currently in progress. Serial judgment theory as it now stands, however, permits such organizational and political extensions to proceed on a more firm behavioral decision theory foundation than has previously existed.

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Rationality, Morality, and Impossibility Theorems

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The impossibility theorem developed by Kenneth Arrow has implications for both rationality and morality in political thought. Transitivity in a collective ordering can be assured only with a decisive set, but this outcome is acknowledged as morally undesirable. The alternatives exhibited by the theorem thus seem to require a choice between rationality and morality. But exit routes can be cut out of this dilemma with the idea of a conditional ordering, one where warranting factors attach to a ranking of alternatives. Conditional orderings form two senses of collective rationality. One is represented by compound directives, which avoid the rational problems of the theorem by warranting local orderings. The second is moral fusion, which requires a reasoned dominance in collective outcomes. These two forms of conditional rationality put into relief the restricted scope of the composition rules and individualism of Arrow's theorem, and suggest alternative relationships of individual and social whole.

Since 1951, when Kenneth Arrow published his brief monograph, *Social Choice and Individual Values,* paradoxes of aggregation have evolved from mathematical anomalies to central problems for a growing number of fields. The attraction of Arrow's theorem for political scientists has been especially magnetic. Though at one time classed as a "minor difficulty" (Dahl and Lindblom, 1953), the cyclical-majority part of the theorem is now pondered in theoretical tracts otherwise unconcerned with aggregation (Wolff, 1970). Only a few students of democracy sensed early the importance of Arrow's contribution (Dahl, 1956). Now even introductory surveys of democratic theory are obliged to worry over the theorem (Holden, 1974). Recent work in political philosophy has even attempted a synthesis of Arrow and Rawls, voiding the problem of the "dictator" by proving that the dominant individual is justified by Rawls' difference principle as the one who has more to lose in the collective outcome (Strasnick, 1977). The results are controversial (Goldman, 1976; Wolff, 1976). At the center of this increasing puzzlement and fresh efforts at resolution is the enormous and expanding body of technical literature that has refined the initial theorem and pointed out its unmistakable implications for political thought (reviews and bibliographies in Plott, 1976; Kelly, 1978).

The conclusions of Arrow's theorem affect two concepts. The first is rationality. Whatever variations have occurred in Arrow's theorem over the years, it is still true that transitivity cannot be maintained in the combined product of individual orderings if a decisive set is to be avoided. Since transitivity is the heart and soul of many conceptions of rationality (Riker/Ordeshook, 1973), Arrow's theorem seems to present a problem for the rationality of social outcomes. The second concept is morality. Transitivity can be assured for a collective ordering if a decisive set is accepted. Or, in the colorful language employed by Arrow, a "dictator" outcome is rational. The dominant individual, however, offends equity. Thus neither outcome of the theorem pleases: the only internal escape from an intransitive ordering is forged at the expense of some basic moral and democratic expectations.

The spectacle of having to settle for either irrational or inequitable outcomes is dreary enough. That recent work has strengthened rather than weakened the impossibility results (Kelly, 1978) emphasizes the intractability of the problem. Yet, in spite of surface appearances, the circle Arrow has drawn is neither tight nor vicious. Exit routes can be cut out of the theorem that enrich rather than restrict both "rationality" and "morality." The assumption of this article is that Arrow's theorem presents important logical and normative problems for recent traditions in political thought. The objective here is to restrict the theorem's importance by laying out alternative senses of collective rationality which avoid the impossibility results of Arrow's theorem while, in one case, fulfilling a more robust concept of morality than recognized by Arrow's axioms and conditions.

The success of this strategy depends on the close fit between rationality and morality in Arrow's theorem. The intriguing hold the theorem has on us begins with logic. Arrow asks us to ponder orderings like these: voter 1 ranks three alternatives $a > b > c$; voter 2 ranks the same alternatives $b > c > a$; and a third voter orders the alternatives $c > a > b$. If these three rankings are combined, an intransitive single ordering results: $a > b$, $b > c$, and $c > a$. The trap is thus set by the famous cyclical majority. We are drawn further into it by Arrow's general theorem. On three axioms—transitivity, pairwise comparisons, and connectivity—and four conditions—universal domain, Pareto optimality, the independence of irrelevant alternatives, and non-dictatorship—Arrow proves in the original theorem that a collective outcome always violates at least one axiom or one condition.[1] The trap is sprung by the proof of a decisiveness lemma (Blau, 1972) which demonstrates that there will always be a decisive set (or dictator, if sets are singletons) under the terms of the theorem if any social outcome is generated from the domain of profiles.

The strength of the trap, however, is not drawn from logical postulates alone. The early lure is the mathematical anomaly of the cyclical majority. But the general theorem must maintain rational and moral requirements to produce the impossibility results. Thus, unlike other well-known paradoxes, e.g., Newcomb's (Nozick, 1969), Arrow's proof does not expose a hard logical contradiction. The paradox is generated by strong, though under-interpreted, views on rationality and morality. Such views are more negotiable than are the rules of logic (on the reasonable expectation that concepts are more easily modified than the law of non-contradiction). The key issues in managing Arrow's paradox are (1) what we are prepared to accept as reasonable specifications of rational and moral outcomes, and (2) what instrument we should employ in reinterpreting Arrow's concepts. The method to be employed here is the introduction of conditional orderings to Arrow's theorem. A conditional ordering will (a) illuminate an alternative form of collective rationality that succeeds without transitivity, (b) introduce a species of collective outcome produced from moral rather than arithmetical combinations, and (c) suggest the tradition of thought within which Arrow's theorem is appropriately located.

The line of argument establishing these conclusions revises Arrow's concept of rationality. But the most damaging restriction of the theorem is imposed by moral considerations. The repugnance of the dictator outcome, necessary for the paradoxical outcome of the theorem and vital to the theorem's importance in democratic theory, can only be sustained from a moral point of view. But the development of morality reveals numerical rather than moral assumptions in the theorem; and a full introduction of morality to the conditions of the proof would require the replacement of arithmetical combination rules with reasoned synthesis. Thus an inspection of one of the concepts necessary to the theorem, morality, leads to a form of collective choice far removed from the defining conditions of Arrow's methods.

Conditional Orderings

A collective outcome in Arrow's theorem is produced by consolidating separate ordinal rankings to a single ordering. This single outcome expresses the ordering for the entire set of individual orderings. The methodological individualism of the theorem is pronounced: starting units are discrete individuals and the social choice is some combination of alternatives ordered by these individuals. The combination rules of the theorem do not address either relations among the individuals or any conditions that might attach to the orderings. To introduce relational or conditional considerations would of course violate the defining features of Arrow's theorem. But such considerations also lead us to a fuller understanding of collective choice.

Suppose the presence of conditionals. Imagine a disconnected society, one where the conditions for rational orderings are segments disjoined, for the moment, simply on whatever considerations produce conditions for orderings. We can at first leave uninterpreted the ordering conditions, representing them with the notations x, y, z. then,

(1) $x \rightarrow (aRbRc)$
(2) $y \rightarrow (bRcRa)$
(3) $z \rightarrow (cRaRb)$.

Or, each of the orderings, (1), (2), and (3), is valid on the terms of each respective condition, x, y, z. Hypothetical case: A set of proposals for managing one aspect of our energy problems is introduced for collective resolution. The issue is a simple (and familiar) one. Available oil does not meet current demands. The proposals are these: a = reduce consumption; b = increase domestic production; c = increase imports. For illustrative purposes, assume what is almost never the case in the real world (but an assumption in Arrow's theorem), that the alternatives (a, b, c) are mutually exclusive. Society is now asked to settle on a policy, or social ordering, of the three alternatives without tradeoffs among any of them.

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[1] I will not present Arrow's theorem here because the sources explaining it are numerous and accessible. To the novice, I recommend beginning either with Arrow's amended original (1963) proof or the more general presentation in Arrow (1967). Kelly (1978) will suggest the scope of variation on the initial version. Among the many interesting symposia on Arrow's work is the one edited by Hook (1967). I will use here the simple notational system employed by Strasnick rather than the more complicated set theory characterizing the use of the theorem in positive theory.

The standard, unconditional outcome for such orderings in the tradition of combined collective choice is the cyclical majority: no ordering can be established for the entire society. But if the validating conditions express a warranted heterogeneity, then a way out of the cyclical majority may be available. The path out of the cycle requires the recognition of a distinction between logical dominance and rational dominance. To simplify matters, suppose two clusters of conditions warrant the orderings of alternatives. Conditions x and y each support a different ordering (as represented by the subscripts–the notational system now following from Strasnick, 1976, 1977):

(1) aR_xb, bR_ya
(2) bR_xc, bR_yc
(3) aR_xc, cR_ya.

The pattern of orderings expressed by (1), (2), and (3) are duplicates of Arrow's combination paradox, with one change: instead of individuals, the subscripts now represent justifying conditions. The orderings can also be expressed as

$x \rightarrow (aRbRc)$
$y \rightarrow (bRcRa)$.

Representative interpretations of x and y can be introduced without difficulty. Say that reduced consumption is justified ($a > c$) when alternative energy sources are readily available and weather is mild (no suffering caused by extreme cold, etc.). Increased imports are justified ($c > a$) when there are no alternative energy sources and weather is harsh. To round out the picture, assume that x and y both contain a clause that permits a priority status for $b > c$, say that new reserves of fuel can be obtained with minimum government support of industry.

The orderings in (1), (2), and (3) produce a decisive ordering when combined, in this case a over c; for the combination of orderings in (1) is $a{-}I{\rightarrow}b$, in (2) it is $b{-}R{\rightarrow}c$ (on Pareto), and for (3) it is $a{-}R{\rightarrow}c$ (on transitivity). If x and y were individuals, x would be the dictator in standard Arrow fashion ($a > c$ dominates $c > a$ in the collective outcome). But x and y now represent conditions, not individuals. As conditions, x and y have a rational relationship unlike the arithmetical patterns of the combined orderings. Suppose that condition y, as a matter of general argument, is persuasive over condition x. On inductive reasoning, the evidence strongly favors condition y as the overriding set of considerations: no effective alternative energy sources exist, the winters look to be harsh, and so on, while no evidence supports condition x. Then, on rational considerations, $y > x$. But on the combined orderings, x logically dominates y as the decisive set. Since $y > x$ on inductive reasoning, and $x > y$ on logical combination, two reasoning systems exist.

Suppose now not only that $x > y$, but that both x and y are valid conditions. Think now of a society heterogeneous on these two conditions, either spatially (e.g., geographical regionalism) or temporally disjoined policy needs over time). Conditions x and y express then an arrangement of conditions justifying different, separate orderings. We have, on the earlier pattern of orderings

(1) $x \rightarrow (aRb)$; $y \rightarrow (bRa)$
(2) $x \rightarrow (bRc)$; $y \rightarrow (bRc)$
(3) $x \rightarrow (aRc)$; $y \rightarrow (cRa)$.

Or, both $a > b$ and $b > a$ on their respective conditions; $a > c$ and $c > a$ similarly on, respectively, x and y; and only $b > c$ is a single expression of social choice. The conditions which justify the preference orderings prohibit adding or combining the orderings. Only the satisfaction of the Pareto condition (a logical expression of spontaneous, as well as unanimous, democracy) produces a social outcome where all choose one or another of the alternatives. Where the Pareto condition is not satisfied, the society requires a variety of different orderings, each valid on local conditions.

The social choice in such pluralistic settings cannot be a single ordering. A compound directive is required if heterogeneity is to be respected. Say (hypothetically) that the sunbelt areas of a society are rich in alternative energy sources, thus fulfilling condition x. Say also that the northern sectors have both harsh weather and restricted energy alternatives, or condition y. Then a rational rule expressing energy policy on the three alternatives can be, "Reduce consumption in the sunbelt areas, and increase imports in the northern sectors." Stated as a formal directive, do (or give priority to) a when x; and do (or give priority to) c when y. The policy is not simple consumer choice. It is a directive policy variable with justifying conditions.

The recognition of compound directives formed from rational, rather than logical, dominance, articulates more fully the nature and limitations of Arrow's theorem. In particular the strong *fusing* effects are put into relief. The general method of aggregation aims to combine separate orderings into a single expression. In voting, the metaphor is familiar: society, though a collection of separate preferences, speaks with a single voice. But the utilitarian collapse of multiple values to a single value is also, in Arrow's theorem, a reduction of reason to logic. The decisive set is a logical rather than a rational dominance. A society pluralistic across justifying conditions, one where different reasons support different preferences in local areas, resists a single ordering on rational grounds. That the theorem imposes one anyway reveals again the indifference of the combination rules to conditional orderings. The compound directive, on the other hand, represents the priority of rational systems in societies heterogeneous on supporting reasons.

The use of compound directives avoids the combination paradox by avoiding combinations. Compound directives are rationally mandated when good and strong reasons divide a society; and the presence of divisive reasons can sometimes favor compound directives on rules of equity, like "Respect local values." Any beginning student of politics knows that a single policy line must sometimes be imposed on disparate views, both for rational and moral reasons. But not always. The strategy, for example, of "disjointed incrementalism" contains a strong emphasis on segmented policies:

> As an illustration, we may ask again whether anyone can say that he prefers, for example, unemployment to inflation or inflation to unemployment when they are in conflict? Some individuals think they know which they prefer, but it can quickly be shown that they cannot arrange even two such values in order of priority. Almost everyone prefers unemployment to inflation if unemployment is small enough and inflation great enough. Yet almost everyone prefers inflation to unemployment if unemployment is great enough and inflation is small enough. One's preferences between unemployment and inflation can therefore be expressed as a set of trades or *as a set of terms* on which one is willingly exchanged for the other (Braybrooke and Lindblom, 1963, p. 88, emphasis added).

Or, in addition to simple tradeoffs, a rational ranking of alternatives can follow the basic form of a conditional: $x \rightarrow (a > b), y \rightarrow (b > a)$. The incrementalist strategy is not unique in accepting conditional orderings. Indeed, the unconditional rankings of Arrow's theorem seem anomalous; the use of conditions, by contrast, appears more nearly the rational norm. The description of collective outcomes as a package of items (a) conditionally justified, and (b) connected internally on meta-rules recognizing local differences, is at least as familiar a sight in the public choice literature as the single-ordering outcomes of Arrow's theorem.

If conditions are contained within orderings, the combination rules of Arrow's theorem can be maintained. But the outcomes may not satisfy the conditional requirements. Strasnick's recent development of Arrow's proof, still relying on the individualistic postulates, introduces a *Social Unanimity Condition*. The condition maintains that "for any set of individual preferences, (a) the weak Pareto principle is to hold for any subset in a partitioning in that set. (b) If for some partitioning, the *SPF* (social preference function) prefers the same alternative in each subset, then it will prefer that alternative for the unpartitioned set of preferences as well" (Strasnick, 1976b, p. 251). This condition is similar to Robert Nozick's "addition condition," which asserts that "if two distributions (over disjoint sets of individuals) are just then so is the distribution which consists of the combination of those two just distributions. (If the distribution on earth is just, and that on some planet of a distant star is just, then so is the sum distribution of the two)" (1974, p. 209).

The "addition condition" requires that a predicate (outcome, value) assignable to all elements of some set, S, will also be assignable to the set consisting of the elements combined. The addition condition is an innocuous re-expression of the Pareto principle when interpreted in terms of unconditional orderings. But conditional orderings defy the addition condition. Suppose, for example, that aRb is a just arrangement when aRc, and bRa is a just arrangement when cRa. Now consider the following orderings:

(1) $aRbRc$
(2) $aRbRc$
(3) $bRcRa$
(4) $bRcRa$.

Each of the partials, (1), (2), (3), and (4), is just on the conditions establishing just distributions. But if we combine them, then we get:

(1) *bRc* (on Pareto)
(2) *cIa* (on the indifference condition)
(3) *bRa* (on transitivity).

The combination of the partials produces *bRa* when *cIa,* an unjust arrangement under the terms set out for the partials. This exercise would be an empty demonstration in set theory were not a number of important concepts interpretable as conditional. "Justice" is among these concepts. Rawls' difference principle (*DP*), for example, is "lexical" in unconnected societies, in a "simpler form" in well-connected societies. Formally, a lexical *DP* requires a sequential satisfaction of worst-off representative persons, a simple *DP* justifies inequalities in general as they benefit the worst-off. A society pluralistic on connectedness will tolerate sets of orderings like those illustrated above, thus requiring multiple senses of *DP*-type justice. Two societies, one connected and the other unconnected, can both be just on different interpretations of the *DP.* Yet adding their distributions will not necessarily produce a sum distribution that is just because of the effects of conditionals on outcomes.

The lines of inquiry marked out by conditionals lead away from Arrow's theorem in two ways. First, conditions may represent reasons that mandate local, not collective, orderings. Such segmented rationalities promise an alternative to the collective rationality of the theorem. Second, the combination rules of the theorem may not dependably reproduce values maintained by conditions. Conditions may be non-additive even when they do not mandate local orderings. The non-additive status of conditions can restrict the power of the theorem to extend normative concepts from individual ordering to collective outcome: justice in the whole may not be identical to justice in parts still added to produce the whole.

The moral implications of Arrow's theorem suggest another line of inquiry on conditionals. We might ask—what, if any, are the consequences of introducing *moral* reasons as conditions for orderings?

The Concept of Morality

Three features of Arrow's theorem delineate moral requirements. First, individuals in the starting conditions of the theorem are equal. No criteria are provided for a priority ranking of individual orderings. (No entitlements nor merit claims are entertained.) Second, the Pareto condition requires that unanimity among individuals be expressed without perversion in the collective outcome. Third, no individual can impose his/her preferences on the entire society (the non-dictatorship condition). Taken together, these three components suggest a familiar picture of morality composed of equality and autonomy (to realize goals without institutional or other-person impediments).

But a different, more elaborate concept of morality can be drawn up from recent moral philosophy. The haphazard career of "morality" in twentieth-century Western philosophy is well known. Early positivists denied truth status to value statements, consigning them either to psychology as expressions of feeling (Ayer, 1946) or to sociology as causal utterances (Stevenson, 1944). The positivist rupture with long traditions of value cognitivism precipitated that familiar modern separation between morality and political science. If moral statements are not truth-functional, then value-neutrality is the proper goal of those seeking verifiable statements about politics (chronicled by Brecht, 1959; Urmson, 1968; MacIntyre, 1966; Hudson, 1970).

More recently, however, philosophical analysis has turned more productive methods on moral language. A rational, and sometimes cognitive, status has been granted to moral statements. Generalized reasons have replaced the older emotive and causal accounts of morality (Hare, 1952, 1965; Toulmin, 1960). Models of hypothetical choice, some rivaling any level of rigor in political inquiry, have been advanced as devices to produce moral principles and rules (Harsanyi, 1953, 1955, 1976; Rawls, 1971). The revival of a rational component for morality introduces again the possibility of a moral-political nexus. We might ask, what features must types of political action have in order to qualify as moral action?

Though contemporary moral philosophy is as heterogeneous as any interesting intellectual field, two features of morality can be found in virtually every recent point of view and a third expresses in moral form the equality recognized by Arrow and long maintained as important for the conditions of moral action.

(1) *Agency: Moral actions are performed intentionally, with volition and sufficient knowledge for responsibility.*

The "internal" quality of moral action that *agency* establishes has been a fixture of moral systems since Plato. The importance of moral credit and blame establish the feature. Various formulae of coercion, ignorance, and absence of intention can excuse or defeat responsibility for action; and moral credit is typically withheld from actions performed under duress, unknowingly, or unintentionally. The role of agency in morality is not free of controversy. Coercing-to-morality is a troublesome issue, made more complicated by recent elaborations of power. The simple model of direct force easily defines out moral predicates: someone beaten into the performance of a good (or bad) deed hardly qualifies as a Good (or bad) Samaritan. But the manipulation of agendas and general conditions is less easily excluded from agency. If, for example, alternatives of choice, *a, b, c,* are restricted to *a, b,* then the agent who chooses $a > b$ can still in some sense be said to have been controlled *and* to have acted intentionally, volitionally, and (in an even more weakened sense) knowledgeably.[2]

The complications of agency in morality do not, however, expel the feature from the concept of morality. A morality without at least a modified sense of agency simply could not be a morality; and the feature coalesces with the next two components to present a reasonably coherent concept of morality.

(2) *Reason-supervenience: Moral directives are warranted by reasons.*

Reasons are needed in moral thought and action because, among other considerations, they separate and support moral alternatives—why one ought to do *a* rather than *b.* A reasonless directive would have nothing to recommend it over other directives. Like agency, however, reasons are complicated items in moral theory. Some have maintained that (almost) anything is a reason if one is prepared to universalize it. On such a view, the freedom to choose basic devices to justify actions is wide, subject only to the test of sincerity—not making an exception for oneself if one turns out to be the victim rather than the beneficiary of the justifying system (Hare, 1965). Others maintain that reasons must satisfy some wider sense of validity, such as causal adequacy, intelligibility, and relevance (Foot, 1958). This latter view weighs the social dependence of reasons more heavily. But the recognition of reasons in moral discourse has, on both views, two important consequences for our understanding of morality.

First, some form of consistency replaces arbitrary actions. The primitive thought that "anything goes" in morality is at least restricted. Though whether elaborations on consistency can (Gewirth, 1978) or cannot (Hare, 1965) arbitrate types of moral principles is unsettled, moral directives are bound by at least some formal requirements of rational discourse. Second, moral accord requires, for its full realization, an agreement on reasons as well as directives. Here the attachment of agency amplifies the point. Think of a moral individual (1) who opposes abortion on reasons drawn from fetal rights. Now suppose a second individual (2), also opposed to abortion but on different convictions: that zero-growth population trends suggest a need for additional births to supply future inductees for the military. Individual (1) will likely be pleased at the no-abortion conclusion of (2), but appalled at the supporting reasons. Individuals (1) and (2) are not in full moral agreement—the moral accord is faulty—because of different reasons for their moral conclusions on what to do. Think of one as the moral instructor of the other. The moral lesson will have been incompletely consumed if the pupil does not understand *why* something is to be done in collaboration with *what* is to be done.

(3) *Equality: All moral agents are equal in their capacity to consume moral goods.*

Equality is often developed as a pair of concepts opposed along a single axis. One pair is (a) a procedural or logical norm requiring similar treatment for similar cases, and (b) a substantive rule of morality or justice requiring equal treatment on certain items (e.g., rights). Another pair is (a) equality of opportunity, and (b) equality of distribution (or outcome). Let us say here only that moral systems presume equal capacity among moral agents for the consumption of goods designated as moral. Examples of moral goods in Western societies include life, certain liberties, guarantees of political participation. Though unequal distributions of such goods do occur, the inequality is not typically justified in terms of diminished capacity among the moral agents so deprived. An inequality in the distribution of economic goods, however, can be defended by unequal capacity (taste, need, preference). Thus the moral status of, say, due process of law is expressed in terms of its assignment to all rational members of society; and the non-moral status of fashionable clothing is indicated both by its divisible allocation and the absence of outrage that some have it while others do not.

Equality is notoriously difficult to maintain and justify as a general principle for social arrangements. But three observations support its central status in morality. First, economic inequalities are frequently conjoined with fixed equality on moral items. Rawls is merely the most recent figure to separate a moral good (liberty—the first principle, and equally distributed) from unequal social arrangements (the difference principle). The tradition of equal justice distinct from economic considerations goes back to Magna Carta and beyond. Second, warranted inequalities in society are often established from a baseline of moral equality. Sometimes unequal economic distributions are formally derived from equal starting conditions (e.g., Rawls' "original position," 1971, pp. 19, 136–42; Harsanyi's equi-probability of being anyone else in society, 1975, p. 598).[3] Even when moral claims must be adjusted to unequal arrangements in the face of competing claims or moral criteria, moral locations are settled by theories of distributive justice on the presumption of initial equality among moral agents (survey in Rescher, 1966). The absence of equal capacity would make unintelligible all such efforts to maintain equality or justify inequalities. Third, institutions frequently recognize the distinctions between moral and non-moral goods by imposing different requirements in each case for justifying inequalities. The United States Supreme Court, for example: while interest-oriented issues are subject only to a "rational relationship" test, when fundamental rights or invidious distinctions are at issue the Court imposes the test of "strict judicial scrutiny." Students of the law learn early that inequalities acceptable on a "rational relationship" test can be judged arbitrary and unwarranted when "strict judicial scrutiny" is applied. Or, equality on moral items (rights, race, national origin) can be overridden only on the strongest of social needs (e.g., "compelling state interest").

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[2] Recent discussions of indirect power by Bachrach and Baratz (1962) and Lukes (1974) elaborate this problem. All who treat the general theme of agenda control, however, are more pessimistic about the undesirable effects of indirect power on moral actions than I am in my brief remarks in the text here.

These three components of morality—agency, reason-supervenience, and equality—are simple devices. The first is necessary to all moral systems, the second to those with rational status, and the third is a condition that has both a long tradition in moral philosophy (Rousseau, Kant, Baier, Rawls) and practical expression in social institutions. They are skeletal items even when combined, in no way providing a fleshed-out system of morality. They are also strongly coherent. Each component draws heavily on the others ("agency" interpreted by "reasons" and "equality"; "equality" depending upon "agency," and so on), so that they form a close and overlapping network of integrated parts not extending powerfully as separate concepts. Yet the small package they provide is an effective device to establish distinctions between arithmetical and moral outcomes in Arrow's theorem.

Moral Outcomes

Think of two alternative actions, *a* and *b*, as the focus of moral choice. If supporting reasons are a defining component of moral direction, then a moral deliberation on *a* versus *b* will explore the reasons supporting each course of action. Though no conclusive status may be assignable to *a* or *b*, a moral choice will select the alternative with the stronger supporting reasons. Let *d* and *e* be sets of reasons supporting, respectively, *a* and *b*. Then $(a > b) \Rightarrow (d > e)$, and $(b > a) \Rightarrow (e > d)$. Example: if a = abortion-on-demand and b = no abortion, then the moral settlement of *a* or *b* will be negotiated through the arguments, rules, principles, utilities, physical states, and so on, which produce sets of persuasive reasons for the alternative proposals.

The introduction of reasons to orderings reveals more sharply the distinctions between types of collective outcomes. Two have been identified so far. The first is the unconditional combination of Arrow's theorem. The second is the compound outcome of conditional orderings. Reasoned orderings are also conditional orderings. But a moral choice cannot be content with the establishment of separate justifying conditions. Compound directives reflect and respect heterogeneous conditions. At issue in a moral outcome, by contrast, is which reason set, *d* or *e*, is rationally dominant. A successful conclusion of moral deliberation would be $d > e$ or $e > d$, with the consequent establishment of an ordering priority between *a* or *b*.

Why? Because of what we mean by morality. If the defining components of morality are to be maintained, a moral outcome is the product of shared reasons among moral equals. Many standard political forms are successful without such demanding requirements. Bargaining, for example, may be consummated without shared reasons. (Each party to the transaction may have a different reason for assenting to the bargain.) Or power may be characterized by coercive resolutions of disagreement over both reasons and actions. Efforts at moral resolution, however, must produce collective outcomes through rational persuasion if an accord on reasons is to be insured. The need to establish a reasoned priority among alternatives marks off moral outcomes as fusing efforts, where, unlike compound directives, the conditions for orderings are themselves the material for orderings (as contrasted with Sen's "ranking of rankings," 1974, 1977).

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[3] The scope of "equality" is demonstrated yet another way with the recognition that Rawls' equality is used to derive what he claims are two non-utilitarian principles of justice, while Harsanyi uses equality to justify, *contra* Rawls, the utilitarian distribution of maximum average utility.

The fusing formulae for reasons, however, are unlike the arithmetical combinations of Arrow's theorem. A collective outcome in Arrow's theorem is produced from counting the ordinal arrangements of alternatives set by individuals. The absence of cardinal scales prohibits adding numerical weights for the alternatives. But a collective outcome is still an arithmetical function of a preponderance of rank-orders. If, for example, $a > b > c$ and $b > c > a$ are "summed," the $b{-}R{\rightarrow}c$, $a{-}I{\rightarrow}c$ outcomes are simple combinations of the individual ordinal rankings. Reasons, however, cannot be counted or summed. The reasoned determination of abortion versus no-abortion is not a matter of combining ordered units, but of exploring the considerations supporting one course of action versus another. The non-additive logic of reasons is clearly expressed in judicial decisions. Imagine the absurdity of a legal outcome produced simply from counting the number of earlier decisions on an issue. A moral outcome, like a legal outcome, is negotiated through rules, arguments, and evidence, not through numerically collecting rank-orders.

The non-additive logic of reasoned outcomes supports intuitive feelings that morality is not a matter of mere numbers. That a preponderance of individuals endorses a point of view seems never enough, in itself, to establish the morality of the point of view. The hostility of morality to additive formulae is explained by the supervenience of moral outcomes on reasons. But, also, a distinction is suggested between preferences and choices, or even picking and choosing (Reynolds and Paris, 1979; Ullmann-Margalit and Morgenbesser, 1977). Arrow's individuals order alternatives, nothing more. No reasons are advanced to justify the primitive preferences expressed in the ordinal rankings. Moral agents, by contrast, choose on the basis of reasoned considerations. The rational basis of moral choice is revealed through supporting reasons. The introduction of moral considerations to the theorem thus provides a rational feature to the initial orderings, based on choice, that is not present in the unsupported preferences of Arrow's individuals.

Reasons also grant a prescriptive status to moral outcomes not found in additive outcomes. A reason to do *a* rather than some rival alternative is a rational appeal for *all* to do *a*. The universalizing power of reasons has been duly noted in the literature (Hare, 1965; Gewirth, 1978). A reason for an action is prescriptive for a class of relevantly similar agents, never restricted to a particular person or situation. The individuals in Arrow's theorem, however, order alternatives with no normative force on others. Indeed, the orderings are logically independent of one another. A moral outcome, representing a rational dominance of one set of reasons, is unifying—a prescription for all moral agents to act in accordance with the outcome. Arrow's conditions and composition rules produce only a descriptive outcome, one which adequately or inadequately combines an array of logically discrete orderings with no prescriptive force.

If the initial conditions and the additive composition rules of Arrow's theorem are inhospitable to morality, then it is reasonable to inquire about the sources of moral concern in the theorem. A second look, with the components of morality outlined here, suggests that they are imported from outside the basic features of the theorem. The original axioms of the theorem—transitivity, pair-wise comparisons, and connectivity—provide no moral criteria. The moral concerns of the theorem are located in the equality assumption and in two of the initial conditions: the Pareto condition and the non-dictatorship condition. The decisive moral condition—the one which mandates intransitivity by its maintenance—is non-dictatorship. But the question is: why on the assumptions and postulates of Arrow's theory of social choice (or preference) is the imposition of one individual's ordering on another morally undesirable?

The moral repugnance of individual dominance is clear on a recognition of requirements in the concept of morality. Moral direction, as amplified specifically by "agency" and "reason-supervenience," demands an agreement on supporting reasons as well as actions. Dominance, by definition, represents behavioral compliance without an agreement on reasons. Or, as commonly understood, an authority dominates a respondent if compliance is obtained without agreement. (Both "power" and "exchange" can be dominant events—see Baldwin, 1979; also Wolff, 1970, and Reiman, 1972, for "morality" and "authority.") The dictator is morally unsatisfactory because he imposes his will on others against their own reasoned beliefs/convictions. So, though the dictator may secure behavioral compliance, he has not obtained moral accord because he overrides reasoned resistance to his directives. If the respondent, as an agent, shares the dictator's reasons for acting and complies with the directive because of these reasons, then the dictator is no longer a dictator and the requirements of morality have been fulfilled.

This well-known (and abbreviated) account of why dictators are not moral figures, or any other explanation of the immorality of dictatorship, cannot be drawn up from the features of Arrow's theorem. The theorem is too spare for morality. All social or relational attributes are prohibited. Each individual is conceptually isolated from every other. (No interpersonal comparisons occur in the original theorem.) Though the alternatives to be ordered are defined by the universe of available items (on the "feasibility" interpretation of "the independence of irrelevant alternatives"), each individual is simply a mathematically discrete unit, uninterpreted and thus without social content. The only connection these individuals have with one another is by means of the additive combination rules that produce a single joint ordering. The theorem, in short, operates with numerical terms. No trace of human needs, wants, or patterns of reasoning can be found. As a consequence, no moral features obtain—only logical postulates and patterns.

The other visible exceptions to the absence of moral concepts in the defining features of the theorem are the equality assumption and the Pareto condition. But now it is clearer that these assumptions are also primarily numerical or logical postulates, not full-fledged moral axioms. A moral sense of equality would be formed on the idea of moral agents alike in their access to moral goods. The Kantian *noumenal* agent expresses such equality. *Because* of attributes assigned to human figures, in particular those universally shared by all rational agents, equality is accepted as the proper first condition of morality. But Arrow's individuals share nothing but numerical equality. They are equal not as moral creatures are equal, in their shared capacity for moral goods, but as mathematical entities are equal: equal magnitudes under the combining rules of mathematics. There is nothing antagonistic to moral equality in the idea of mathematical equality. The absence of priorities among individuals is an important formal component in morality. But equality in mathematics is not moral equality until the features of moral agency are attached. The same points can be scored against the Pareto condition. The non-perverse expression of unanimity in outcomes is without moral status because the expression fails to reflect whatever reasons may support the correspondence of orderings. A mathematical, rather than a moral, accord is insured by the Pareto rule. Like equality, the outline of a moral concept is present in the Pareto condition. But the outline is not the concept.

Moral concerns imported from foreign sources are, of course, still moral concerns. But foreign imports can also bring in foreign values. Such values are difficult to resist if a theorem requires external concepts as a consequence of its internal logic. Arrow's decisive set is not a dictator without moral considerations; and the non-dictatorship condition can be sustained only on an endorsement of moral points of view. Moral terms, however, force a modification of the theorem. Morality requires that rational agents order alternatives for reasons. But if reasons are accepted into the theorem, then the basic combination rules of the theorem must give way; for reasons, as we have seen, are non-additive considerations. And if the combination rules are replaced by argumentative synthesis, then the theorem is gone. The rich implications of moral outcomes can now be appreciated. Moral reasoning is not a solution to Arrow's paradox, for its employment requires a rejection of some basic conditions of the theorem. But a major condition, the resistance to dictatorship, is moral, not logical; and this resistance is precisely what produces the intransitive ordering in outcomes. Thus the paradox that the theorem exposes is parasitic on the acceptance of a moral point of view. What we have seen is that this moral point of view is undeveloped within the theorem because of the mathematical/logical form of the theorem's defining features. Once this moral point of view is developed, however, it imposes novel lines of inquiry away from the basic ideas of collective choice found in Arrow's theorem.

Heterogeneity and Fusion in Social Choice

Arrow's theorem is the apotheosis of unconditional combinations. Two types of conditional outcomes, however, satisfy the requirements of collective rationality. One is the compound outcome that authenticates local orderings by means of meta-rules. The other, represented by moral reasons, fulfills the incomplete moral concerns of the theorem. Several conclusions follow from the recognition of these two alternative types of collective outcome.

(1) Conditional orderings are closer in spirit and logic to the terms set out by Arrow than are many other alternatives to the theorem. Conditionals do not introduce intensity mea-

sures (e.g., Borda scales), holistic terms that impose settlements from the collective to the individual level (e.g., the state as an organic whole), or any of the relational factors that give up the simple (atomistic) individualism of the theorem (e.g., custom, social rules, statute law, etc.). The conditions introduced here are attached to individual orderings conceptually distinct from each other and ordinally arranged.

(2) Yet, in spite of the affinity of conditionals for the general framework of the theorem, their introduction qualifies the methodological individualism on which the theorem is based. Arrow's individuals are atomistic units, arranging alternatives from isolated values. Social outcomes are the combined products of individual rankings. But if individuals, as individuals, rank on conditions, then the standard aphorism of methodological individualism—"the whole is equal to the sum of its parts"—permits emergent values, and is not even always true. Wholes may be unrepresentative sums, failing to reproduce values found in constitutive parts. Or wholes may be outcomes from non-additive composition rules.

(3) The magic of collective rationality without arithmetical combinations is accomplished in two quite different ways with the use of conditional orderings:

> (a) The compound directive gives up both transitivity as a measure of collective rationality and the composition rules of the theorem. Since the outcome of Arrow's theorem is a single ordering for the entire set of individuals, transitivity is, understandably, a feature of this ordering. Compound directives are meta-rules authenticating multiple orderings on different warranting conditions. Each conditional ordering is transitive. But they are not combined. Thus there may be no single transitive ordering representing the collective outcome. The outcome, however, is no less rational in the absence of transitivity. Indeed, if pluralism is warranted, then the imposition of a single ordering would itself be irrational.
>
> (b) A moral outcome maintains transitivity but gives up the composition rules. Arrow's theorem produces (when transitivity is maintained) a logical rationality not equivalent to an outcome dominant on persuasive reasons. When the concept of morality is introduced to the theorem, the outcome is like Arrow's outcome in the singularity of the final ordering, but unlike Arrow's outcome in its rational rather than logical dominance. Indeed, a truer sense of collective rationality is achieved through reasoned synthesis than through the arithmetical combinations of Arrow's theorem. The outcome can express a transitive ordering *and* be the product of reasoned deliberation.

(4) Both uses of conditional orderings introduce more complex and interesting senses of rationality than found in Arrow's theorem. The compound directive stresses the rationality of heterogeneity in opposition to combinations. Moral outcomes emphasize reasoned outcomes. But of the two, moral considerations promote the stronger alternatives to Arrow's version of collective choice by calling into question the notion of an "individual." Morality interprets this term beyond its primitive logical status in the theorem, enriching it with human agency. Various efforts have been made to expand the individual in Arrow's theorem. Prominent among these expansionist efforts is the moral notion of "extended sympathy" (Sen, 1977; Arrow, 1977). But if the arguments here are correct, at the end of all moral extensions of the individual are new senses of fusion and collective outcomes.

(5) Neither development of conditional orderings is a solution to the paradox Arrow offers us. No surprise here, for a really good paradox should be airtight to begin with—no escape as long as the defining terms are maintained. The strategy employed here has been to restrict and put into relief the scope of the theorem while staying as close as possible to its rational and moral dimensions. The result of such a strategy should, if successful, disclose the location of the theorem in a pattern or tradition of thought. No less has been accomplished here, though requiring more speculative observation.

Arrow's theorem, in its definitions and rules, expresses a relationship of the individual to the society that has dominated political theory at least since the seventeenth century. Some have identified the basic social form as a market model of possessive individuals (Macpherson, 1967, 1973, pp. 25–31). The reliance on simple preferences, unconstrained and without justifying reasons, is certainly a part of any market model. But the more basic form is that of a collection of autonomous individuals constituting a society with no features beyond those provided by combining the individuals as discrete units. The legacy is Hobbes, Mill, and, especially, Bentham.[4] Arrow's theorem represents the logical extremes of that tradition, its distinctive assumptions and rules of social order transformed into a rational and moral dilemma.

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[4] A representative statement in the tradition of the numerical society, by Jeremy Bentham (1832): "The community is a fictitious body, composed of individu-

A longer tradition puts into narrow relief the singular individualism of this legacy. Aristotle conceives of individuals as inseparable from the *polis,* and the *polis* as an extension of common understandings among individuals. The lines of inquiry away from the theorem lead as much to this classical past as to the present. The organic society of Aristotle's political philosophy suggests exactly those alternative relationships of individual and society that avoid both the rational and moral dilemmas that Arrow's theorem so powerfully demonstrates. That at least one fusion of the individual with society can fulfill moral as well as rational concerns is an important disclosure, not least to those main lines of recent democratic thought that have for centuries relied on the individualistic assumptions of Arrow's theorem.

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al persons who are considered as constituting as it were its members. The interest of the community then, is what?—the sum of the interests of the several members who compose it. It is vain to talk of the interest of the community, without understanding what is the interest of the individual" (p. 3).

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Aristotle, Kant and Rawls on Moral Motivation in a Just Society

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John Rawls considers his Theory of Justice *to be in the Kantian tradition. Generally there seems to be agreement among Rawls' critics that at least with respect to the procedural formulation of the principles of justice, it is difficult to call Rawls' position Kantian. In this article I will argue that Rawls' Kantianism is best understood as providing a motive source for acting upon known just standards of conduct. In this regard Rawls can be read as synthesizing aspects of Aristotle's* Nicomachean Ethics *and Kant's moral reasoning to provide the rationale to explain why an individual who knows what is morally correct conduct in a given situation, makes such knowledge the source of his action. Demonstrating the Aristotelean roots of Rawls' Kantianism with respect to the problem of motivation for just conduct helps one understand how Kant's moral theory can be viewed in Rawls' words not as a "morality of austere command but ... [as] ... an ethic of mutual respect and self esteem" (1971, p. 251). Secondly, this view of Kant provides the basis for understanding the anti-corporatist aspect of Rawls' political theory that my reading of Rawls makes necessary.*

John Rawls (1971, p. 257) considers his *Theory of Justice* to be in the Kantian tradition. Generally there seems to be agreement among Rawls' critics that, at least with respect to the procedural formulation of the principles of justice, it is difficult to call Rawls' position Kantian.[1] Kant's conception of practical reason holds that reason must determine ethical principles of conduct apart from considerations of inclination and interest (1948, pp. 71–74). Rawls' notion of the original position derives principles of right conduct from within a thought context that makes interest and inclination paramount objects of concern. Rawls' original position is a state of mind in which individuals choose principles of conduct from behind a veil of ignorance (1971, pp. 136–42). In the original position one is assumed to lack knowledge of the conditions of one's life in society: one is unaware of one's potential opportunities, talents, goals, chances of success, etc., and must choose principles of conduct that would be best for oneself, given the intention to maximize the possession of primary goods (Rawls, 1971, pp. 17–22). The primary goods such as liberties, rights, income, powers, and self-respect are the resources which people use to realize their personal life plans (Rawls, 1971, p. 92). Persons engaged in moral deliberation from within the perspective of the original position try to assume a neutral posture toward the various interests they know constitute the domain of human motivation. Consequently, people in the original position do not conceive of the principles of justice with the intention of giving themselves an advantage in attaining any specific interest, but rather they seek to find principles which would insure ease of access to any one of the diverse interests that they might, once outside the original position, assume for themselves. Interest considerations are thus primary matters of concern, and instead of a Kantian categorical imperative (a rule of morality that should be observed for its own sake, since it is a statement of the most moral position acceptable to rational people (1948, pp. 69–71), Rawls derives a hypothetical imperative in the original position. A hypothetical imperative takes the form that if *y*, the intended interest, is known, then an *x*, or a correct means, must be used to achieve *y* (Kant, 1948, p. 82). Conceived in these terms, Rawls' principles of justice are understood as means to achieving any one of a wide variety of interests that one could choose once outside the original position in everyday life.

But as Dworkin argues (1979, p. 43), the original position is allowed to take on "too much of a life of its own in [*The Theory of Justice*]." Consequently, the part of the *Theory of Justice* that explains the motivational

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[1] See especially Johnson (1974), Darwall (1976), and Levine (1974). Rawls' most important replies to the critics of his Kantianism are found in Rawls (1975, 1977).

environment conducive to supporting principles of justice is often overlooked. Rawls himself anticipates this problem when he writes that

> The motivation of the persons in the original position must not be confused with the motivation of persons in everyday life who accept the principles that would be chosen and who have the corresponding sense of justice. In practical affairs an individual does have knowledge of his situation and he can, if he wishes, exploit contingencies to his advantage. Should his sense of justice move him to act on the principles of right that would be adopted in the original position, his desires and aims are surely not egoistic. He voluntarily takes on the limitations expressed by this interpretation of the moral point of view (1971, p. 148).

Thus even when one has knowledge of what constitutes just conduct for a given situation, unless a motivation exists (what Rawls refers to as a "sense of justice") to act upon this knowledge, the individual could be moved by self-interested considerations totally in contradiction to moral knowledge.

I will argue that Rawls tries to explain the motivation for just conduct outside the original position, and in doing so Rawls, unlike Kant, clarifies the nature of the motivation for support of moral precepts in day-to-day life. In this regard, Rawls can be read as synthesizing aspects of Aristotle's *Nicomachean Ethics* and Kant's moral reasoning to provide the rationale to explain why an individual who knows what is morally correct conduct in a given situation, makes such knowledge the source of action. This discussion is important for several reasons. First, demonstrating the Aristotelean roots of Rawls' Kantianism with respect to the problem of motivation for just conduct helps one understand how Rawls could describe Kant's moral theory not as "a morality of austere command but . . . [as] . . . an ethic of mutual respect and self-esteem" (1971, p. 256). Secondly, this view of Kant provides the basis for understanding the anti-corporatist aspect of Rawls' political theory. In particular, Rawls' well-ordered society is a form of pluralism that synthesizes communal ideals with individual liberty while avoiding the impediments of modern-day interest-group liberalism as understood by Lowi (1969) and others (such as Garson, 1971).

In the following article, the first section will discuss Kant's moral vision and the difficulties he faces in discerning a motivation for moral conduct. The second and third sections will discuss Aristotle's moral vision, especially as it pertains to Aristotle's understanding of friendship and to Rawls' use of this view of friendship to provide motivational content to his own ethical theory. The third section will also discuss the difference between Rawls and Kant in relation to a motivation for just conduct. The final section will discuss the type of pluralism suggested by my view of Rawls.

Kant's Moral Vision

In Kant, the motivation to conform activity to ethical principles is best understood from the standpoint of the relationship between reason and desire. Rational men define universal rules of conduct, called categorical imperatives (Kant, 1948, pp. 69–71). Necessarily, these rules must be conceived apart from matters of interest or inclination; otherwise, they would merely serve as rationalizations for self-interest. These rationalizations portray self-interested concerns as containing a moral validity of their own, and this is a contradiction which no rational person could accept. Consequently, reason must discern universal principles of right conduct (moral maxims) without regard for any particular end or desire at issue in a given situation where the moral maxim is considered relevant (Kant, 1948, pp. 67–68). Moreover, even when the question of the purity of moral knowledge is put aside, one finds for Kant (1948, p. 63) that reason is an ineffective faculty for realizing the goals of desire at times when these goals are paramount and legitimate objectives. It is far better to follow one's instincts to achieve ends rooted in desire and interest, for instinct is nature's means for realizing the twin goals of happiness and pleasure.

One should not interpret, as some have,[2] the dichotomy between reason and instinct to imply an irresolvable conflict between the forces of morality as contained in the moral maxims and the forces of desire as known by the drives for pleasure and happiness. Actually, Kant can be construed as claiming that life should be understood from two different, but integratable perspectives. In one viewpoint human life is understood as belonging to the sensible world. From this perspective the individual operates within the context of the laws of nature which chart the relationship between the structure of desire, the empirical environment in which the desires are elicited, and the behaviors that seem to follow as necessary correlates of the effort to achieve the goals posited by desire (Kant, 1966, pp. 153–71; 1948, pp. 118–21). In the other (noumenal) viewpoint, human beings conceive of themselves as free from external, determining factors. From this perspective the individual uses the power of reason to discern the concepts that put the ideas of sense under a meaningful set of rules. In particular, the individual has the power to organize his life so it is in keeping with the moral laws reason posits for all rational beings (Kant, 1966, pp. 194–202; 1948, pp. 118–21). Thus the individual who understands the impact of the laws of nature or the sensuous world on his life can arrange his life in terms of the moral perspective he chooses as a rational and free agent, without causing tension between the two viewpoints.[3] For example, anyone who is likely to succumb to improper sexual advances should strive, if choosing to act in accordance with the moral maxims concerning sexual conduct, to avoid situations in which such advances are likely to be made.

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[2] See, for instance, Sullivan (1974) and see Wolff (1973) for a refutation of views like Sullivan's.

But what would motivate a person to follow a course of action in which the synthesis of the two viewpoints occurred? Especially, why would anyone seek a moral synthesis where the motivation of the empirical self for happiness is so compelling and yet where the motivation of the noumenal self seems to be much less tangible? For Kant (1948, p. 68), the motivation to abide by moral laws is the knowledge that these laws themselves are the most valid, most moral form of conduct one can choose. Such knowledge presumably breeds reverence and respect for the moral law. Having acquired this understanding, one can become a free human being, able to make moral choices and live with the dignity which proper moral actions bestow on men.[4] Thus knowledge of moral truths alone should be the foundation for an interest in moral laws, and, as Kant says, once a person recognizes the existence of such laws, "[a person] must necessarily take an interest in it [moral law]" (1948, p. 117).

Nonetheless, it is difficult to understand how an interest in morality could derive solely from a recognition of moral laws themselves. Unless the individual is firmly committed to a goal toward which the moral laws contribute, it is unclear what practical motivation a person would have for abiding by moral laws. Of course, to introduce a goal toward which the moral maxims are considered contributory would appear, at first, to violate the idea of the moral laws as non-instrumental and as truths independent of any end. Nonetheless it is conceivable that resort to the categorical imperative can contribute to desirable results, results which in fact are so overwhelming and pleasing in nature that people would naturally accept them. When Kant (1948, pp. 95–96) introduces the maxim to "act in such a way that you always treat humanity, whether in your own person or in the person of any other, never simply as a means, but always at the same time as an end," he suggests an end, posited by reason (and not desire) toward which human conduct should aim and toward which moral maxims should contribute.

This maxim, no doubt, requires one to act toward others from the standpoint of universal rules of ethical conduct. But will people be motivated to do so if they fail to appreciate other human lives for the unique qualities and characteristics that constitute human existence? It is my view that support for the maxim to treat others as ends is always dependent upon peoples' appreciation of other human lives as embodying those qualities deserving of respect. When this awareness exists, the goal toward which universal moral rules contribute (to treat others as ends) is given concrete form. Further, it is almost axiomatic that when the maxim to treat others as ends is defined in terms of concrete and tangible possibilities of real human lives, the goal toward which moral rules contribute is clarified and the motive for supporting such rules is strengthened. Consequently, as a crucial step in creating the conditions conducive to encouraging a motive for supporting moral conduct, it is necessary that the maxim to treat others as ends be given a material form.[5] If this maxim were realized, what sorts of objectives would be pursued and within what types of experiential contexts would pursuit of such objectives take place?

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[3] I infer this position from the following statement in Kant, realizing of course that my view is an inference not supported by a direct statement. "We see now that when we think of ourselves as free, we transfer ourselves into the intelligible world as members and recognize the autonomy of the will together with its consequence–morality; whereas when we think oursleves as under obligation, we look upon ourselves as belonging to the sensible world and yet to the intelligible world at the same time" (1948, p. 121). For a supporting interpretation, see Galston (1975) and Wolff (1973, pp. 118–38).

[4] In this regard Kant says (1948, pp. 114–15), "All concepts, therefore, and with them all principles, though they may be possible *apriori* refer nevertheless to empirical intuitions, that is, the data of a possible experience."

[5] For an excellent approach to this question, see Auxter (1979, pp. 125–32).

In the next section I will introduce Aristotle's notion of private and civil friendship. Throughout the discussion I will show those sections of Aristotle's writings which can provide Kant's thought with the motivational support for his concept of moral laws, and I will also argue that Rawls' thought represents a synthesis of Kant and Aristotle. By following this path I will claim that Rawls successfully meets the criticism leveled at him by Bloom (1975, p. 656), who argues that "there must be an interest in morality, just as there is an interest in money or food and one which has primacy over all other interests. Rawls has done nothing to establish such an interest." To the contrary, Rawls' particular synthesis of Aristotle and Kant provides the motivational element otherwise lacking in Kant's moral thinking, and in doing so Rawls explains how an interest in morality outside the original position is possible in the day-to-day world.

Aristotle and Friendship

Aristotle in the *Nicomachean Ethics* (1156a5-1156a24) discusses three types of friendship, but, as Cooper suggests (1976, pp. 634–39), the three types fall into two categories: pleasure/advantage-friendships and character-friendships. The first type is predicated on a sharing of benefits (Cooper, p. 636). Each party in a friendship sees in the other qualities that are useful for attaining a particular end. Human relationships must be balanced by episodes of fair exchange. Thus a person in an exchange relationship to another expects to receive benefits which are approximately similar in worth to those a person gives. Otherwise, exchanges would be unreciprocated and unfair and the relationships predicated on these exchanges would break down (1132b32–34). Consequently, friendships of the pleasure/advantage type require each party to live up to the terms of an exchange, for no one wants to be in a relationship to another in which one gives benefits but receives nothing of approximate worth in return.

A character-friend, as Cooper says, "loves his friend because of properties that belong to the friend essentially. This means that he loves him for what he himself is, and not for merely external properties, or relations in which he stands to other persons" (1976, p. 640). Friendship of this sort is based on a person's respect for the moral excellence exhibited in another person's life. Thus character-friends will always appreciate the depth and excellence of each other's life, they will wish each other well, and they will be able, in Aristotle's words (1172a7–8), to "share in those things which give them the sense of living together."

Character-friendships are predicated on an appreciation of human beings per se, and they do not, as Cooper says, "take place within so restricted a context as that imposed by the self-centered desires for pleasure and profit that operate in the other types of friendships" (1976, p. 640). Nonetheless, character-friends could still have advantage-relationships without damaging the mutual appreciation each has for the other as a human being. Thus two friends might help each other achieve certain goals, but neither would permit these exchanges to subvert the basic commitment to human excellence (particularly as manifested in each one's life) that insures the enduring quality of the friendship.

Cooper rightly assumes that Aristotle believed that both types of friendship are crucially important to each other. At the level of exchange friendship, people perceive their relationships in terms of the pleasures or benefits each provides the other. To be stable and balanced, relationships of this sort must provide fair and reciprocal exchanges of benefits or pleasures. But friendship can be established on an even sounder basis which incorporates interest concerns without at the same time making these matters the sole basis of the friendship. Thus in discussing why people form associations, Aristotle says that people

> help each other by throwing their peculiar gifts into the common stock. It is for these reasons that both utility and pleasure seem to be found in this kind of friendship. But this friendship may be based also on virtue, if the parties are good; for each has its own virtue and they will delight in the fact (1162a25–28).

Aristotle's view of friendship harmonizes the concerns of interest with the requirements of morality and community. Consequently, Aristotle says that the old proverb, "'What friends have is common property' expresses the truth; for friendship depends on community" (1159b30–32). Without this connection to community, friendship would remain only an abstract idea, lacking the experiential depth of an actual relationship. But within a community the self is nurtured in an environment that sustains in people a real sense of friendly feelings and well-wishing for others.

But even though the self is other-oriented, it still is not so thoroughly absorbed in others that it fails to have both knowledge of and respect and even love for the unique capacities and virtues which characterize its own existence. In fact, if the self lacked virtue or if it could not love and respect any virtues it did have, it appears that the individual would fail to develop the concern for others that community encourages. For Aristotle, "Friendly relations with one's neighbors, and the mark by which friendships are defined, seem to have proceeded from a man's relations to himself" (1166a1–3). An individual who has positive regard or love for himself, because his life embodies rational and good qualities, will hold friendly feelings toward others with similar good qualities. Thus for Aristotle the "extreme of friendship is likened to one's love for oneself" (1166b35–38). By contrast, a man who is motivated by irrational and self-destructive goals will not be capable of having friendly feelings or doing friendly deeds because his lack of respect for himself makes it impossible for him to respect others. In this case the individual would lack the ability to integrate the moral concerns of community with individual interests.

It would seem then that the extent to which one has self-respect would determine the extent to which one could unite the two types of friendship in a single personality. For instance, the individual who received his sense of self-respect from an enduring commitment and knowledge or moral virtue would be a person who fully understood the essential properties and moral excellences of human life. Such an individual would, by extension, seek to support and celebrate the same excellences in others, including those known personally and those known only by general reputation. A person of this sort could have advantage-friendships too, but would never allow advantage considerations to dominate personal relationships. Instead, such a person would value and support all those who exemplified human excellences, whether or not such individuals benefited him directly.

The self in Aristotle contains an ability to wish others well and an intense concern to realize a variety of interests and needs. Friendly relationships in general encourage a synthesis between these dimensions of an individual even though the empirical forms of actual friendships vary with different associations (1161b8–19; 1160a28–31). Consequently, while the basic purpose of friendship may remain the same from association to association, the actual form which friendship takes varies greatly. Being a special form of community, political community requires a special form of friendship.

In a political community citizens who have different needs and interests believe that they can benefit or profit from the common membership in the community. Aristotle says that men join political communities to "provide something that they need for the purposes of life; and it is for the sake of advantage that the political community too seems both to have come together originally and to endure, for this is what legislators aim at, and they call just that which is to the common advantage" (1160a10–14).[6] Civic friendship makes possible a supportive environment for advantage relationships by existing as a form of well-wishing toward and even an ability to sacrifice for other citizens (1169a22–35, 1166b30–1167a2). Civic friendship is thus an important motivational component for support of those standards of just conduct which society defines as the necessary rules for upholding an environment in which citizens find a common advantage.

Logically it is possible for a citizen to make a commitment to just standards outside an atmosphere of friendship and well-wishing to others. But in reality this commitment is most evident within the context of friendly relations. Certainly friendship naturally embodies just standards. Aristotle says, "The demands of justice also seem to increase with the intensity of the friendship, which implies that friendship and justice exist between the same persons and have an equal extension" (1160a6–9).[7] Thus if friendship implies any single, common rule of behavior, it is surely that no person ever treats a friend unjustly. Moreover, friendship does not require that one measure and weigh each aspect of the relationship against a precise standard, always being certain that justice in the most formal sense is being served (Cooper, 1976, pp. 646–47). A concept of just standards could be supported in an environment devoid of friendly feelings, but when this situation occurs it would seem that the tolerance and warmth implied by friendly relationships would give way to austere and harsh formal social encounters. No doubt this prospect would be destructive to respect for just standards, since without friendly feelings there would be no strong motivation to wish others well, and in this case no strong feeling to uphold just standards would be apparent.

[6] Also see 1129b31–1130a1 and 1167b5–10, the Principle of Unanimity. In the *Politics*, Bk. 4, especially 1289a26–1299a30, is also useful.

[7] With regard to the relationship between friendship and justice, see Winthrop (1978).

In this regard, good will is crucial to civic friendships. Good will is a respect and well-wishing for others based upon the good qualities that one understands others to possess. "In general, goodwill arises on account of some excellence and worth, when one man seems to another beautiful or brave or something of this sort. . ." (1167a18–20). But while good will is not the same as friendship, it is the first step toward the existence of friendship (1167a4–5). As Aristotle says (1167a3–4), good will is to friendship "as the pleasure of the eye is [to] the beginning of love." Good will does not require anyone to actually befriend another, but it does provide a motivational dimension in which people are inclined to become friends and in which all members of society are permitted and encouraged to support standards of justice that allow individuals to develop those excellences which make them worthy of friendship. Without good will, the trust and tolerance necessary to respect the lives of others is lacking, and without trust and tolerance it is unlikely that we extend to those we do not know personally the same standards of justice that we automatically extend to our friends. Consequently, for Aristotle, in relation to any regime, "Each of the constitutions may be seen to involve friendship just insofar as it involves justice" (1161a10–11).[8]

Rawls, Aristotle, and Kant

Rawls' theory seems to use a concept similar to Aristotle's notions of friendship and self-respect. Rawls argues that "other things equal, human beings enjoy the exercise of their realized capacities (their innate or trained abilities) and this enjoyment increases the more the capacity is realized or the greater the complexity" (1971, p. 426). An individual gains self-respect by using "mature faculties" to perform activities that are "appreciated and confirmed by others who are likewise esteemed [by other association members]" (1971, p. 440). Individuals who engage in activities which permit them to utilize "mature faculties" not only gain self-respect but they engage in activities that are supportive of others as well. Thus Rawls argues that an individual of this sort "generally contributes to the rational plans of our associates" (1971, p. 425). Moreover, it should be noted that the "contribution" is not solely the result of what we referred to above as advantage-relationships but that the "contribution" stems from character-friendships too. For, as Rawls says (1971, p. 425), people who exercise realized abilities are likely to gain support from their associates because their associates "are likely to take pleasure in the display of human excellence." Linked with a sense of self-respect gained from the realization of "mature capacities" is a disposition to look upon others not merely from the standpoint of the concrete manner in which they contribute to private interests, but also from the standpoint of how others manifest the highest, most noble qualities of human activity. Consequently, as in Aristotle, a sense of concern for others is associated with a strong sense of self-respect.

Relationships between individuals are predicated on agreements to uphold mutual obligations incurred in the course of making exchanges with each other. Rawls' principle of "fairness" (1971, pp. 112–13) merely restates this idea by requiring individuals to do their part as defined by the rules of institutions when two conditions are met: "First, the institution is just (or fair), that is, it satisfies the two principles of justice, and second one has voluntarily accepted benefits of the arrangement or taken advantage of the opportunities it offers to further one's interest." But the principle of fairness would not in itself be realistic or practical unless it were a part of an individual's disposition to live in accordance with an appreciation for the accomplishments and triumphs of others. Without the character dimension built into the disposition of individuals, advantage-relationships might quickly develop destructive tendencies in which people left relationships as soon as they received what they wanted from them.

Rawls attempts to blend both dimensions of friendship into the personality of the well-ordered citizen. This approach is necessary in the context of Rawls' efforts to sustain a general sense of well-wishing for others. The desire to wish others well is always associated with the civic perspective which assumes that other people are as concerned with our welfare as we are concerned with theirs. Rawls' moral psychology argues then

> that the active sentiments of love and friendship, and even the sense of justice, arise from a manifest intention of other persons to act for our good. Because we recognize that they wish us well, we are for their well being in return. Thus we acquire attachments to persons and institutions (and must presume standards of justice) according to how we perceive our good to be affected by them (1971, p. 494).

Similarly, without an atmosphere of well-wishing, it would seem highly unlikely that a material and emotional motivation to act upon known just standards of conduct could endure. Support for principles of justice presupposes more than knowledge of just standards, but support always presupposes a motive embodied in civic attitudes to uphold them. As Rawls says, "It would seem that while the individual understands the principles of justice, *his motive for complying with them for some time at least, springs largely from his ties of friendship and fellow feeling, and his concern for the approbation of the wider society*" (1971, p. 473, emphasis added).

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[8] For additional reading on this issue, see Hardie (1968, pp. 317–36).

Rawls and Kant approach the problem of motivation differently even though both share the view that standards of justice are necessary to inhibit destructive social conflict. Kant views society as composed of competing groups and interests, and recognizes that at times the competition may be destructive. As Reiss claims (1970, p. 20), for Kant laws are needed to establish "criteria by which we can settle public conflicts or interests. The principle of universality demands that our social and political relationship should be sound and our conflicts settled in a universal manner." Similarly, for Rawls the interests of the sub-units of society often run counter to each other and conflict of the most harmful kind is often possible. Therefore a "morality of principles" must be invented to define "right not in relationship to group interests or to individual interests but to a concept of right choices irrespective of contingencies" (1971, p. 473). The quest to find universal solutions which are acceptable to rational individuals and which transcend interest conflicts is as apparent in Rawls as in Kant. Both hope that when solutions of this kind are found, a social equilibrium sufficient for human flourishing can be provided.

Kant's approach to the problem of motivation moves only partially along Rawlsian lines, and then veers sharply away. Thus in his *Lectures on Ethics* Kant argues that people should have a moral training that orients emotions and appetites to favor right principles. But in describing the substance of an emotional life conducive to right action, Kant seems to empty it of empathetic emotions that might be conducive to the support of moral principles. Kant claims that a disposition to act may be a psychological state devoid of passion and emotion, and he furthers this stark view of moral psychology when he describes the properly motivated individual as "thorough," "regular" and "resolute" (1963, pp. 146–47). In keeping with this view, for Kant emotional states such as friendship are seen as counterproductive to morality. Kant claims that "the complete moral perfection of heaven must be universal; but friendship is not universal; it is a peculiar association of specific persons; it is man's refuge in this world from his distrust of his fellows, in which he can reveal his disposition to another and enter into communion with him" (1963, p. 207). Consequently, in contrast to both Aristotle and Rawls, Kant's friendship leads people to treat matters of broad social importance parochially, and it makes people distrustful of those outside one's sphere of friends.

Ironically, for a philosopher seeking an ethics based on freedom and reason, Kant seems to settle for a Hobbesian-type motivation for support of moral laws. In discussing the motivation for nations to support moral laws, Kant concludes that nations will abide by moral laws only when it is clear to them that if they do not, they will suffer in the ensuing international conflict (1970, p. 189).[9] Thus while Kant would hope that motivation for moral laws could be founded in an environment approaching the one Rawls describes, it is clear that his pessimistic perspective entails a starker, almost utilitarian explanation (much to Kant's own distaste, of course) for the motivational support for morality.

Rawls' understanding of the source of moral interest helps to meet the criticism that Kant's philosophy is merely formal and abstract, having little practical significance for the real problems human beings face (Riley, 1973, p. 460). Rawls balances Kant's reliance on law with the claim that people take an interest in morality when they are a part of social relationships which embody universal principles and which allow people to treat others as ends in a concrete sense. Rawls' moral psychology attempts to demonstrate how this task can be achieved (1970, p. 189).[10] This article cannot examine the validity of Rawls' moral psychology, but suffice it to say that Rawls hopes to use this approach to explain how people are habituated to a pluralism based on all persons having a moral concern to accommodate their needs to the variety of individual and group interests in society. Rawls' pluralism as the "social union of social unions" can be viewed as a material reconstruction of Kant's concept of the Kingdom of Ends (1948, pp. 100–02).

[9] Also see Galston (1975, p. 239).

[10] See also Rawls (1977, pp. 160–62; 1975, p. 548).

Rawls' Pluralism

Rawls' pluralism is best described in terms that specify the types of social groups and the relationships groups have with each other. In this regard Rawls' well-ordered society permits a variety of associations. Some groups permit purely individual activity, with the individual entering the group to accomplish certain goals and leaving when those goals have been realized and all duties and obligations have been met. Other groups allow individuals to become part of a collective enterprise, providing a sense of community. As Rawls says:

> Increasingly it becomes more important to secure the free internal life of the various communities of interest in which persons and groups seek to achieve, in modes of social union consistent with equal liberty, the ends and excellences to which they are drawn. In addition men come to aspire to some control over the laws and rules that regulate their association, either by directly taking part themselves in its affairs or indirectly through representatives with whom they are affiliated by ties of culture and social situation (1971, p. 543).

Social unions of this kind never deny rights to members, even if the preservation of these rights contributes, on occasion, to the loss of group cohesion. During times when group unity is threatened civic friendship helps to preserve the group's integrity.

Furthermore, certain types or forms of groups would be more acceptable than others. There is no reason to doubt, for example, the viability of participatory groups in which workers choose their management and define company goals or policies. In this type of society, organizations that did not permit individuals to have rights against the groups or against each other would find a hostile reception. Similarly, in the context of the well-ordered society, individuals and associations would have to be organized to accommodate themselves to interests beyond their own group (Rawls, 1971, pp. 467–69). No single group would necessarily be the central focus and unifying theme in an individual's life. In fact, such a totally encompassing community could be perceived as jeopardizing individual freedom and autonomy. Thus associations could provide only a partial focus to an individual's life, and even then these associations would materialize only when an individual himself defined his own focus from within his own "life plan."

This social vision would conflict with present-day corporate models of social organization associated with interest-group liberalism. Modern pluralism, or what can also be called interest-group liberalism, threatens Rawls' well-ordered society. Individuals in this type of corporate society form or join organized groups to acquire political and economic strength to protect their private interests, interests which they may hold in common with a selected population. These groups are sub-units of the whole society and are usually strategic to the economy or to the political process. In classic interest-group theory, there are a variety of groups competing with each other for a share of the basic resources that guarantee power (Truman, 1951, pp. 26–33). It is assumed that the competition leads to a dispersal of power across the diversity of groups, and thus no single group or coalition of groups dominates all major policy decisions (Dahl, 1971; Lipset, 1962, p. 36). The state is only to be an arbiter among interests (Oakeshott, 1962, pp. 37–58).

But the countervailing powers argument often does not accurately depict interest activity and influence. In fact, certain groups sometimes dominate and become oligopolies in their sphere of interest, acting as a type of "private government," making policies which protect the group's own interests while ignoring the public welfare (Lowi, 1969, p. 295). Furthermore, as Lowi says, "The more clear the official practice of recognized interests, the more hierarchy introduced into the society" (1969, p. 88). The group's success is contingent upon its ability to organize itself along oligarchial and authoritarian lines. Decisions are centralized, power is concentrated in a few hands, and the leadership becomes remote from the members. Lowi points out that "we need no more search to support Michel's iron tendency toward oligarchy in these private governments" (1969, p. 88).

Thus when corporate organizations force individuals to subordinate themselves to a set of bureaucratic imperatives as part of the goal to maintain their power in society, society is less able to provide its members with a broad variety of groups flexible enough to accommodate particular individual excellences. An individual still committed to a Rawls-like vision would challenge a corporatist outlook and seek instead to act from moral principles that, if realized, would accommodate an authentic pluralism. Individuals of this sort are still capable of appreciating human excellences across a broad spectrum of people and interests. Moreover, because these types of people would most likely befriend each other on the basis of a mutual respect for human excellences, character-friendship would still remain a possibility even in the heart of corporate society. Character-friends would create a sense of community (and civic friendships) among themselves and celebrate the diverse excellences that their lives personified. Civic friendship in this context is an experience conducive to an intense commitment to moral principles which undergird a yearning for justice.

Conclusion

In conclusion, Aristotle's conception of the *polis* combines justice and friendship. As Mulgan writes (1977, p. 14), for Aristotle, "friendship, a wider concept than in modern English, implies a general sociability, a desire to cooperate in shared activity of any sort, from the utilitarian business transaction to the close, personal relationship of true friends." Kant strove for a dutiful application of moral principles, a commitment that would help us restrict the baser influences of our inclinations and emotions. Rawls' notion of the well-ordered society holds that in a large diverse society it is necessary as Kant argued, to construct a set of principles common to all rational people. But Rawls also maintains that both a commitment to these principles and to the society sustaining them is possible only in an atmosphere of general sociability as described by Aristotle and modified by Rawls. For without this atmosphere, an interest in doing good—that is, an interest in morality itself—might not exist. Rawls' contribution then to Kant is to show that friendship as described by Aristotle is not alien to morality, and indeed if Kant's thought is to gain moral force, this dimension must become an integral part of Kant's moral vision.

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Policy Choices and Regional Equality among the Soviet Republics

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While socioeconomic equality and maximum economic growth have been the proclaimed aims of Soviet leaders, the simultaneous implementation of these goals is difficult. Analysis of budgets reveals that expenditures in economic categories vary significantly across Soviet republics, and decline little over time. The republics are substantially more equal in socio-cultural expenditures, and are becoming even more so with time. The level of urbanization accounts for a large share of the variance in the social budget but not the economic one. Policy decisions concerning the latter are likely subject to a process involving a variety of political, military and socioeconomic considerations that transcend both the equality and efficiency criteria. In contrast, the increasing parity among Soviet republics in the socio-cultural budget is closely related to the increasing urbanization of the republics at the lower end of the development scale. It appears that increasing regional equality in the Soviet Union may be achieved primarily through improvements in social opportunities, and not by means of direct redistribution of economic resources.

Economic growth and sociopolitical equality have been identified as essential aspects of the processes of modernization and political development (Binder et al., 1971). The achievement of growth and equality is also the proclaimed aim of leaders in modernizing and modern societies. These values are clearly evident in communist systems, where the ideological world view stresses an egalitarian distribution of benefits, and where the need to assure the success of socialism in a hostile world has led to an emphasis on rapid economic development. Communist leaders often declare their pursuit of the twin goals of economic growth and the elimination of socioeconomic inequality within their states, and claim these goals as the basic guides to policy making. The fact is, however, that the goals of growth maximization and equality are sufficiently diverse to make them often competitive rather than complementary policy norms. Members of the political and economic elite must make some priority choices between the political objective of egalitarianism and the economic goal of maximum national development.

How have the goals of growth and equality influenced policy in the Soviet Union? Have there been significant variations in policy preferences over time? Similarly, are different goals evident across diverse socioeconomic endeavors? This article attempts to answer these questions by looking at the spatial dimension of the problem, analyzing the pattern of regional differences among Soviet republics to ascertain the relative impact of the two objectives, growth and equality, in regional policy making.

The Equality and Efficiency Objectives

Pursuing the goal of equality necessitates, among other things, the elimination of socioeconomic differences among Soviet republics and the eventual equalization of economic benefits and social opportunities in the USSR. This has been an official policy goal since the founding of the Soviet state (*Pravda,* 1961; Brezhnev, 1972), and in December 1972, CPSU General Secretary Brezhnev claimed that "in the main" this goal had been accomplished in regard to the economic development of the union republics. In turn, removing the inequality meant that economic issues could thereafter be approached on the basis of country-wide interests. Brezhnev's statement suggests, by implication, that the pursuit of inter-regional equality was not synonymous with efficient and rational growth of the national economy.

This disparity stems from the fact that the two goals, equality and maximum growth, each necessitates different uses of economic and social resources. Providing similar benefits and services across the country requires an allocation of resources in favor of the less-developed areas, enabling those regions to obtain a higher

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rate of return from the initially larger investments (Koropeckyj, 1972, p. 68; Schroeder, 1973, pp. 190–93; Zwick, 1976, pp. 501–05). Such a strategy of regional development is counterproductive to the goal of maximum growth for the national economy as a whole (Koropeckyj, 1972, 1973; Whitehouse, 1973). The latter objective is best served by a policy which obtains maximum returns per unit of investment, signifying highest resource allocations to the most economically developed regions.

Economists argue that the most efficient use of production forces is achieved through the concentrated development of already existing social as well as economic facilities (Holubnychy, 1973, p. 29; Koropeckyj, 1972, p. 68; 1977, p. 112). Benefits such as easy access to housing and transportation, and the provision of good health care, educational opportunities, and cultural programs are factors that contribute to greater economic efficiency. In this view, the availability and quality of such services affects productivity, and therefore, the rate of return on investments. Accordingly, to achieve the most efficient use of allocations for infrastructures and services, planners should improve the existing externalities in the developed regions rather than distribute social benefits across all regions.

There are considerable disagreements among planners in socialist societies about this type of policy, precisely because of the ideological commitment to equality. Even specialists who argue for the necessity of short-term regional economic variations to assure maximum economic growth insist that similar differences cannot be tolerated in the social sphere, and that equity in socio-cultural services must be provided by the government (Koropeckyj, 1977). This means that different criteria should determine social and economic decisions, with the goal of equality prevailing in social outlays, and efficiency in economic ones.

Another argument rejects the dichotomy between efficiency and equality as criteria for social financing. Echols (1975, pp. 260–61) suggests that social investment, as opposed to economic allocations, may result in a higher rate of return in the less developed areas. In this case, the more effective distribution of social resources would be towards investment in the backward areas, with no conflict between the goals of efficiency and equalization.

Policy choices regarding regional development in the Soviet Union are clearly subject to a variety of considerations. While it is true that Soviet planners are given guidelines for the location of resources, these standards reflect the varying and sometimes competitive policy priorities of the Soviet leadership. In locating industries, for example, planners are told to take into account such factors as the maximum use of natural, infrastructural and labor resources, defense requirements, as well as to aim toward the equalization of economic and living standards, and the elimination of urban-rural differences (Hamilton, 1973, pp. 236–39; Wagener, 1973, pp. 66–67, 80). In part, the vagueness of the guidelines may be attributed to the decision makers' recognition of the complexity of trying to achieve maximum economic growth and equalization at the same time. Soviet specialists have recognized that this is an aim requiring a multifrontal solution (Kosygin, 1971; Telepko, 1971). The pursuit of both efficiency and equality cannot be subject to an orthodox process of economic maximization emphasizing strictly efficiency criteria. Instead, decisions are likely to be based on a suboptimal combination reflecting the twin aims of equalization and growth.

Data and Methods

The policy outputs examined here consist of budgetary expenditures at the Soviet republic level in various economic and social sectors.[1] The budgetary process represents one of the most systematic procedures through which policy choices and policy changes can be examined in socialist states (Bunce and Echols, 1975; Welsh, 1975; Bunce, 1976). The ability of Soviet leaders to implement political and economic goals through the allocation of funds is enhanced by the centralized and hierarchical organization of communist planning and management systems (Pryor, 1968, p. 44). Fiscal transfers can be made to redistribute monetary allocations from one sector to another, or from one region to another, depending on the desired objective. This is not to imply that fiscal choices are translated into corresponding levels of outcomes. On the contrary, as has been shown by studies of Western systems, the distribution of expenditures does not necessarily lead to the same provision of goods and services (Sharkansky, 1967). Thus I want to stress that I draw no inferences here from the budgetary data to the actual distribution of material benefits among the Soviet republics.

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[1] Not all of the expenditures for industry in the Soviet Union are channeled through the republic state budgets (Tulebaev, 1969). Industrial capital investments would be a better data source; unfortunately, such data are available in too aggregate form to be of use in this analysis. While reliance on the budget expenditures for industry is thus problematic, since we have no exact knowledge as to how the amounts reported in the budget relate to the whole of industrial outlays, the fact is that in the centralized system of socialist planning the state budget is bound to reflect the goals set for the entire economy (Pryor, 1968, p. 44).

Rather, the primary concern of this study is to assess the relative impact of the goals of equalization and economic efficiency on cross-regional expenditures. The dependent variable has been operationalized as budgetary expenditures in economic, socio-cultural and welfare areas at the Soviet republic level for the 1957–1970 period.[2] This approach enables a systematic comparison of cross-sectoral similarities and differences and of temporal trends in the selection of policies for Soviet regional development.

I have chosen Soviet Union republics as units of analysis both because of substantive issues and the availability of data. Soviet republics are most relevant to an assessment of the economic efficiency and equalization patterns because it is at this level that the issue is most visible and most politically sensitive (Silver, 1974a, 1974b; d'Encausse, 1978). Furthermore, the 1972 claim by Brezhnev of the Soviet achievement of regional equality pertained to the national republics, making the study of these units especially important. The longitudinal data for the period 1957–1970 also permit comparisons between the Khrushchev and Brezhnev regimes, an analysis of potential importance in view of Bunce's (1976) conclusion that changes in political leadership have a significant bearing on policy outputs.

The independent variables consist of the levels of urbanization and of employment in industry for each republic. Both Soviet and Western observers claim that many of the interregional differences evident in the USSR can be traced to the variations in the extent of urbanization among the republics (Zlatin and Rutgaizer, 1969, p. 20; Schroeder, 1973, p. 190; and Zwick, 1976, p. 519). According to this view, the diverse availability of funds, goods and services in different parts of the country reflects the urban-rural dichotomy. The best possible explanation for variations in expenditures, therefore, is the respective level of urbanization in each republic or region—a proposition that can be tested by regressing the dependent budgetary variables with the percentage of urban population in any given area.[3] The critical importance of the urbanization variable is further attested by the fact that Soviet documents have cited urbanization as a central criterion used in the determination of resource allocations (Koval, 1973).

Inclusion of the variable, percent of labor force in industry, is related to the importance attached by some scholars to the allocation of resources to industrial areas in order to obtain the most efficient and maximum economic growth. The prevalence of this "economies of agglomeration" view can be measured by looking at the amount of variance explained by both independent variables, that is, republic urbanization and industrial employment levels. It should be noted that Koropeckyj (1972, pp. 71–72) found a strong relationship between industrial employment and industrial output in any given area, indicating that the former variable represents a region's level of industrialization and economic development in general.

The following empirical analysis of Soviet budgetary expenditures at the republic level proceeds in a twofold manner. First, I look at the actual degree of equalization for the various policy outputs to determine how the norm of equalization was pursued in the Soviet Union in the 1957–1970 period.[4]

I turn next to determine whether variations in expenditures can be explained mainly by the use of the efficiency criteria for the maximum growth of the economy as a whole, irrespective of the ensuing regional inequalities. The above discussion has shown that such a policy would best be served by favoring urban and industrial areas with monetary allocations. The lack of a trend toward equalization, especially in economic investments, would suggest that this indeed is the pattern of regional development in the USSR.

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[2] The budgetary data analyzed here are in current prices. Their comparability is assured since all sub-national units use the same fiscal reporting methods, and since we are looking at yearly distributions across regions.

[3] All coefficients reported in this study were obtained through a lagged regression between the dependent and independent variables; that is, the levels of urbanization and employment in industry for a particular year was regressed with the budgetary data for the following year.

[4] To measure the size of differences among republics I rely on the coefficient of variation (CV). The CV is the standard deviation over the mean, and is therefore a standardized measure permitting comparisons of distributions with different means. Of course the CV is only a measure of dispersion, and not of the direction of dispersion. Discussion of whether the pattern of inter-regional inequality is skewed in favor of the more or less developed republics must thus be based on both the CVs and the correlation (*r*) coefficients (Tables 1–2).

Equalization Trends

There are several views concerning the levels and directions of equalization at the Soviet republic level. Brezhnev's statement about attaining essential parity among Soviet republics reflects the Soviet verbal claims that expenditures for social and economic benefits are fairly consistent across the country. In contrast, Western economists argue that Soviet leaders are concerned foremost with maximum efficiency in both economic and social sectors, and therefore neglect equalization in favor of continuing priorities across all the budgetary outlays to the most urban-industrial republics. Another view discriminates between the economic and social sectors, claiming that while the efficiency norm is prevalent in the former area even at the cost of increasing equality, the same is not true of the social infrastructure. The reason for this difference is either that efficiency considerations give way to political necessities of equalizing social opportunities, or that the rate of return per unit of investment in social areas is more beneficial in the less developed regions than in their more advanced counterparts. The expectation here is that equalization among Soviet republic budgets will be evident in the social categories but not the economic ones.

The data in Table 1 show the pattern of equalization in the 1957–1970 period. Turning first to a comparison across sectors, we see that the coefficients of variation clearly demonstrate significant differences between the economic and social fiscal areas. The summary measures for both sectors, that is the total amounts for per capita expenditures on economic and sociocultural activities, respectively, reveal that the actual size of the variation among union republics is twice as large for the economic budget than for social service.[5] (The range for the latter CV is from .209 to .154, the CV for economic outlays varies from .481 to .308). Disaggregating these categories into their various component parts reveals that the same pattern holds when we compare the financing of the economy with spending in education and health. It does not hold, however, for social services directly related to the state's welfare function. Programs of social maintenance and welfare support are characterized by a highly unequal distribution of funding; in fact, interregional variations in these areas are, in general, among the highest. The reasons for these dispersions, however, are different. Funds for state assistance are the only ones among all the budget categories which exhibit a negative correlation with urbanization (Table 2). This holds true for the entire 1960s decade, meaning that the more rural republics have consistently dispersed a greater share of state welfare funds than the urbanized republics. The pattern is explained by the fact that a large share of these outlays goes to aid large families, whose demographic location is primarily in rural areas. The social maintenance budget, on the other hand, is highly correlated with the level of urbanization (r is continually above .84). Funds from this category go primarily to social programs which favor workers, thereby accounting for the predominance of these fiscal outlays in urban-industrial republics.

These findings lead to several important observations. First, the "maximum" interpretation of the efficiency argument, the concentration of funds in the more developed areas across all economic and social programs, is not supported by the data. The consequences of such a policy would be a high degree of inequality on all budget lines, a transpiration not observable in the data. From this it appears that the Soviet elite has not sought to implement an exclusive efficiency norm and give preferential treatment across the board to the more advanced republics in the hope of obtaining a maximum return on their allocations.

Rejection of the efficiency criterion as an overall policy norm does not mean that the growth objective is not applied in some sectors of the budgetary process. Indeed, the pattern of the coefficients of variations across the budget areas discloses that the "minimal" version of the efficiency-as-policy norm view may be operational. The argument here is that the highest differential in terms of returns from resource allocations in developed and underdeveloped regions is found foremost in the industrial sector, followed by infrastructures, but that in social services no such differences are evident. The policy of equalization could then be applied most readily in the latter area, while industry and closely related concerns would continue to exhibit a relatively unequal distri-

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[5] The economic and socio-cultural categories are the totals for expenditures in their respective areas, including the other budgetary outlays presented in this study. However, while all of the economic sub-category are provided in the tables, the physical culture sub-category is excluded from the socio-cultural side of the table.

Table 1. Coefficients of Variation for Budgetary Expenditures Per Capita by Soviet Republics, 1957–1970

Year	Total Economy	Industry and Construction	Agriculture and Procurements	Transport and Communications	Housing and economy	Total Socio-Cultural	Education	Health	Social Maintenance	State Assistance
1957	.395	.440	.698	.499	.438	.200	.152	.172	.520	NA
1958	.356	.534	.531	.479	.446	.206	.162	.163	.537	NA
1959	.395	.490	.751	.503	.419	.209	.167	.160	.497	NA
1960	.404	.480	.793	.439	.371	.184	.160	.148	.480	.477
1961	.481	.578	.946	.474	.369	.181	.160	.140	.472	.524
1962	.429	.502	.785	.504	.366	.179	.150	.137	.478	.573
1963	.463	.467	.932	.497	.379	.178	.147	.142	.475	.614
1964	.409	.465	.751	.464	.408	.172	.144	.135	.476	.665
1965	.335	.425	.572	.499	.438	.163	.140	.129	.474	.705
1966	.323	.541	.396	.506	.415	.174	.146	.125	.468	.749
1967	.318	.493	.390	.494	.412	.170	.163	.121	.473	.786
1968	.319	.479	.398	.416	.395	.161	.153	.123	.473	.825
1969	.308	.497	.392	.479	.398	.166	.173	.128	.471	.848
1970	.393	.373	.488	.426	.380	.154	.152	.111	.440	.884

Sources: Gosudarstvennyi Biudzhet SSSR i Biudzhet Soiuznykh Respublik (Moscow, "Finansy," 1963, 1966, and 1972 editions) and from the annual publications of *Nardone Khoziaistvo SSSR* (Moscow, "Statistika").

bution among the Soviet republics. This interpretation is supported by the data in Table 1 (excluding the social welfare variables). Thus, in general, in the period under examination here, the highest amount of inequality among the union republics exists in industry and transport and communications, areas most directly relevant to rapid economic growth. The variance declines for expenditures on communal infrastructures, and is markedly lower for the outlays for education and health. The data on the degree of variance among republics supports the contention that equalization is least likely in areas bearing directly on material production, but because of a lesser absorption of costs vis-à-vis optimal use, is more likely to occur in the socio-cultural sectors.

The fact that there has been more equality in socio-cultural expenditures, as well as in education and health, throughout the 1957–1970 period suggests that the Soviet inter-republic development plan has been directed toward the creation of equal social conditions. At least in relation to the provision of regional fundings in economic sectors, more egalitarian standards have been established for

Table 2. Correlation Coefficients (*r*) for Budgetary Expenditures Per Capita Lagged with Percent Population Urban by Soviet Republics, 1957–1970

Year	Total Economy	Industry and Construction	Agriculture and Procurements	Transport and Communications	Housing and Economy	Total Socio-Cultural	Education	Health	Social Maintenance	State Assistance
1957	.51	.50	.21	.37	.74	.87	.60	.82	.84	NA
1958	.61	.45	.30	.41	.66	.87	.59	.82	.86	NA
1959	.55	.49	.16	.45	.72	.86	.67	.79	.87	NA
1960	.49	.50	.15	.44	.74	.86	.64	.76	.86	–.35
1961	.37	.39	.08	.44	.77	.86	.63	.76	.87	–.38
1962	.43	.42	.16	.45	.73	.87	.66	.73	.87	–.38
1963	.37	.51	.08	.47	.68	.86	.62	.74	.88	–.39
1964	.36	.40	.12	.44	.64	.86	.61	.71	.88	–.41
1965	.49	.43	.18	.50	.66	.87	.62	.72	.88	–.43
1966	.43	.06	.32	.49	.67	.86	.59	.76	.88	–.43
1967	.41	.003	.37	.51	.67	.84	.50	.78	.88	–.48
1968	.42	.12	.21	.49	.62	.85	.47	.77	.89	–.50
1969	.49	.12	.30	.46	.58	.84	.52	.79	.88	–.55
1970	.48	.59	.24	.55	.62	.84	.46	.70	.85	–.56

Sources: Gosudarstvennyi Biudzhet SSSR i Biudzhety Soiuznykh Respublik (Moscow, "Finansy," 1963, 1966 and 1972 editions) and from the annual publications of *Nardone Khoziaistvo SSSR* (Moscow, "Statistika").

the disbursement of money in the primary social services. This policy has resulted, at the republic level, in comparable expenditures for social overhead rather than for economic activities. This supports Echols' (1975) argument that parity among the union republics can be achieved through socio-cultural development. This approach may signify a long-term perspective on the attainment of equality in all material and social benefits. This is true especially if the share of expenditures is much larger in industry than in social services, an opinion held by Western economists who claim that the socialist countries give little "attention to the development of social overhead and of other than industrial sectors of the economy in general . . . despite official claims to the contrary" (Koropeckyj, 1973, p. 56). The basic question is whether a relatively more egalitarian funding of socio-cultural than economic activities provides the basis for an overall equalization of inter-republic differences. While a complete answer cannot be given because of the limited data available, the creation of more equal social opportunities is likely to result in socioeconomic mobility. In turn, this would create a more productive labor force in the backward regions and, consequently, increase returns per unit of economic expenditures. The overall effect, in the long term, may very well be to establish conditions making the underdeveloped republics more competitive in terms of economic growth. The labor shortage in the Soviet Union makes a shift from the strained urban centers to the heretofore rural areas, with their underutilized labor force, especially attractive.

The thesis that the policy choice of Soviet leaders and planners is principally directed toward immediate social, not economic, equalization is further supported by the temporal trends in variations among republics. Returning to Table 1, we observe a steady pattern or consistent decline in the coefficients for expenditures in the socio-cultural categories (with the exception of state assistance, where rising inequality favors the more rural republics), but considerable variations on the economic side of the budget. Again, this indicates a commitment to norms leading to the creation of more equitable social opportunities in the union republics. The steady compression of differences in the socio-cultural budget denotes that annual funding on this dimension has gradually closed the gaps among Soviet republics.

This does not occur for expenditures in industry and related infrastructures. Instead, the coefficients vacillate up and down throughout the 1957–1970 period, indicating a lack of any systematic budgetary policy for these categories. Monetary dissipation is slanted more toward the less industrialized republics in some years than in others, resulting in a trend toward greater equality. At other times, the pattern is reversed and the more developed republics spend a greater per capita amount in the economic field than their less-developed counterparts. This brings about an increase in inter-republic differences.

I deduce from this data that the policy standards employed in the disbursement of economic funds are not principally geared toward equalization or maximum growth; at the very least neither is a consistent feature in this aspect of the budgetary process. Most probably other considerations intervene in economic investments, contributing to an idiosyncratic pattern on the equalization measure. This is not the case for the social-cultural budget, where the constant decrease of inter-republic differences testifies to an equity commitment which transcends structural and leadership changes. Thus on the socio-cultural measures, the pattern in favor of equalization is evident during both the Khrushchev and Brezhnev periods, and consequently during the decentralized and recentralized phases of the Soviet management systems. As concerns economic expenditures, in general, it is possible to state that the various structural changes toward decentralization in the late 1950s/early 1960s did not lead to a leveling of inter-republic variations and, rather, may have contributed to a slight widening of differences in all sectors except housing and communal economy. Given one of the purported aims of these reorganization schemes, greater autonomy in the economic field for horizontal planning agencies, this is not an unexpected result since the better-off areas would be in a stronger position to resist redistributive finances. The small change toward increasing equalization during the Brezhnev period may be owing to the reassertion of a vertical management authority as well as to the new leadership's intensified commitment to equalization.

The striking difference between the Khrushchev and Brezhnev periods is in the agriculture and procurements budget. The coefficients of variation prior to 1964 are overwhelmingly above the .75 range, while in the second half of the 1960s they are predominantly at .39 (Table 1). This change can be accounted for by the different agrarian policies of the two regimes, and specifically by Brezhnev's abandonment of Khrushchev's virgin lands program. The development of new, unexplored lands in the eastern regions of the Soviet Union required substantial investments which resulted in a highly unequal distribution of agrarian funds. In Table 2 the correlation coefficients for levels of urbanization and disbursement in agriculture almost double from the years of Khrushchev's to Brezhnev's leadership. This testifies to the fact that after 1964 the funds which were previously allocated to the underdeveloped regions, with "virgin lands" programs reverted to the more developed areas, but since agricultural funding naturally favors rural regions, this redistribution accounted for a decrease in inequality on this dimension.

In summary, the patterns on the equalization of inter-regional differences in the Soviet Union support the earlier findings of Echols (1975) and Bunce (1976). The coefficients of variation for the 1957–1970 period demonstrate that expenditures in the diverse economic categories vary significantly across republics, and furthermore (with the exception of agriculture), show little evidence of declining over time. In contrast, the levels and temporal direction of the coefficients for the social policy budgets reveal that union republics are substantially more equal in the socio-cultural fiscal outlays and are becoming even more with time. On the basis of these observations, we can conclude that the equalization policy norm has been operative in the social services area and that increasing parity in social finances has been one of the results of this policy. In the economic area, large inter-regional differences remain among the union republics. It remains to be seen whether the primary reason for this distinction can be attributed to an overriding concern with economic efficiency and maximum national growth.

Efficiency, Equality and Policy Choices

The finding that equalization among Soviet republics is relatively greater for social expenditures than for economic ones, in conjunction with the argument that highest returns on fiscal outlays are obtained in urban-industrial centers, leads to the expectation of much higher associations between levels of urbanization and economic budgets than between the urban and social expenditure variables. Such a pattern would indeed signify that considerations of efficiency prevail in the economic financing of the union republics, resulting in a substantial degree of per capita budgetary inequality among them. The correlation and determination coefficients in Tables 2 and 3, however, show that this is not the case.

The data in these two tables reveal that, in most instances, the degree of association between urbanization and budgetary levels is more significant for the social-cultural categories than for the economic expenditures. A cross-sectional comparison for the period under examination here discloses that the correlation coefficients range from the .70 plus figure for fiscal lines for socio-cultural expenditures, health and social maintenance, through a figure above .50 for housing and education and coefficients below that for the economy, indus-

Table 3. Coefficients of Determination (r^2) for Budgetary Expenditures Per Capita Lagged with Percent Population Urban by Soviet Republics, 1957–1970

Year	Total Economy	Industry and Construction	Agriculture and Procurements	Transport and Communications	Housing and Economy	Total Socio-Cultural	Education	Health	Social Maintenance	State Assistance
1957	.26	.25	.04	.14	.55	.76	.36	.67	.71	NA
1958	.37	.20	.09	.17	.43	.75	.35	.67	.74	NA
1959	.30	.23	.02	.20	.51	.73	.44	.62	.76	NA
1960	.24	.25	.02	.19	.55	.74	.41	.57	.74	.12
1961	.13	.15	.01	.20	.60	.75	.40	.58	.76	.14
1962	.18	.18	.03	.21	.52	.75	.43	.53	.75	.14
1963	14	.26	.01	.22	.46	.73	.38	.55	.77	.15
1964	.13	.16	.01	.19	.41	.74	.37	.51	.77	.17
1965	.23	.19	.03	.25	.43	.76	.39	.52	.77	.19
1966	.19	.00	.10	.24	.45	.74	.35	.58	.78	.19
1967	.17	.00	.14	.26	.45	.71	.25	.60	.79	.23
1968	.18	.01	.04	.24	.38	.72	.23	.59	.78	.25
1969	.24	.01	.09	.21	.34	.71	.27	.62	.78	.30
1970	.23	.35	.06	.30	.39	.71	.21	.50	.73	.32

Sources: Gosudarstvennyi Biudzhet SSSR i Biudzhety Soiuznykh Respublik (Moscow, "Finansy," 1963, 1966, and 1972 editions) and from the annual publications of *Nardone Khoziaistvo SSSR* (Moscow, "Statistika").

try, agriculture and transportation sectors. The only negative correlation is between urbanization and state assistance financial provisions. This has already been explained by the fact that most of the monetary support in the later category goes to large families, whose demographic location is predominantly in the rural areas.

These differential associations between urbanization and the social and economic expenditures, respectively, need to be viewed in the perspective of the prior findings on equalization. The differences among republics in sociocultural expenditures are relatively minor. Variations, however minimal, still exist and most of this variance can be accounted for by the different levels of urbanization—at least for the summary social measure and health, as well as for the relatively greater inequality prevailing in social maintenance. On the other hand, the significant variations that were observed for the economic budgets are not explained by inter-republic differences in the percentage of urban population. Essentially, then, while the small amount of variance in social service outlays can be attributed to the urban-rural dichotomy, the same cannot be said for the more substantial variance in regional economic expenditures. It should be noted that the data using the percent of labor force in industry as the dependent variable did not provide for a higher amount of explained variance; in fact, in the main, the correlation and determination coefficients were lower than for the relationship between urbanization and budgetary outlays.

Differences in public expenditures in economic sectors, therefore, cannot be accounted for primarily by industrial employment and demographic data. Other variables need to be examined to explain a larger share of the variance in financing the economy. These are likely to reflect significant political, military, social and economic considerations in the allocation of industrial and related resources. Political pressures from some important vested regional interests, security requirements for the dispersion of key industries, natural resources found in the remote parts of the Russian republic, or the availability of labor supply can all influence the planning and budgetary processes (see Duevel, 1973). Tradeoffs between these demands and the policy norms of efficiency and equality are apparent in Soviet regional development, making neither the economic maximization nor the political parity objectives the primary guidelines of decision makers for fiscal policies in the economy.

The relationship between the distribution of economic expenditures across the republics and the independent variables is also temporally unstable. As Tables 2 and 3 indicate, there are fluctuations in the proportion of variance accounted for by urbanization. This reinforces the interpretation that varying criteria are most influential, at different times, in the determination of disbursement for the economic sectors. The most striking change during the 1957–1970 time span occurs for industrial expenditures, where the amount of variance explained by the urban levels drops to near zero for the 1966–1969 years (Table 3). This is especially surprising given the fact that at the time administrative control over the industrial planning system reverted to the ministries in Moscow, which are more likely to allocate funds in regions assuring highest overall economic growth. While no definite explanation can be provided here for these low coefficients, it is possible as Hamilton (1973) argues, that Soviet planners in this period attempted to rank smaller cities and less developed areas higher, relative to urban centers, in the spatial development of industry. In any case, this preference appears to have declined by 1970, when the correlation between urbanization and industrial expenditures went back up to .59 (Table 2). The use of urbanization and percent of labor force in industry as the independent variables reveals that the previously described trend in the industrial budget is not duplicated (Table 4). In fact, the major difference in r^2's between Table 3, where urbanization is the sole dependent variable, and Table 4, where both levels of urban population and industrial employment are regressed with the budgetary policy outputs, is found for expenditures in industry for the 1966–1969 period. While at other times and for other sectors the labor force variable does not add significantly to explanations of variance in expenditures, it does so for the anomalous years in the industrial category. It appears, then, that during this time Soviet planners were allotting budgetary outlays to a greater extent to areas relatively non-urbanized but possessing some form of industrial base. It is likely that industrial development was then favored in remote areas of the Soviet Union endowed with partially explored natural resources, and that the greatest push for the further development of these areas occurred between 1966 and 1969.

The evidence derived from Tables 2, 3 and 4 on the relationship between the independent variables and social budgets demonstrates a more stable temporal pattern than for the fiscal economic sectors. There is a very consistent level of association for the overall socio-cultural category and its health and social maintenance

Table 4. Coefficients of Determination (r^2) for Budgetary Expenditures Per Capita Lagged with Percent Population Urban and Percent Labor Force in Industry by Soviet Republics, 1958–1970

Year	Total Economy	Industry and Construction	Agriculture and Procurements	Transport and Communications	Housing and Economy	Total Socio-Cultural	Education	Health	Social Maintenance	State Assistance
1958	.49	.39	.36	.21	.59	.78	.35	.68	.85	NA
1959	.43	.44	.36	.24	.59	.76	.44	.64	.86	NA
1960	.47	.55	.33	.21	.62	.75	.41	.59	.85	.58
1961	.55	.56	.46	.24	.66	.76	.40	.60	.84	.55
1962	.46	.46	.41	.21	.56	.76	.44	.57	.82	.58
1963	.38	.48	.31	.22	.46	.75	.39	.59	.83	.57
1964	.37	.45	.26	.19	.41	.76	.38	.56	.83	.60
1965	.42	.58	.28	.25	.43	.77	.39	.56	.84	.61
1966	.27	.49	.30	.24	.45	.74	.37	.61	.84	.66
1967	.18	.52	.21	.26	.45	.71	.25	.63	.84	.70
1968	.18	.53	.12	.24	.39	.73	.23	.62	.84	.70
1969	.24	.43	.20	.21	.34	.71	.28	.67	.83	.72
1970	.25	.37	.22	.30	.40	.73	.22	.58	.79	.75

Sources: Gosudarstvennyi Biudzhet SSSR i Biudzhety Soiuznykh Respublik (Moscow, "Finansy," 1963, 1966, and 1972 editions and from the annual publications of *Nardone Khoziaistvo SSSR* (Moscow, "Statistika").

components. The state assistance funding denotes an increasingly negative correlation with urbanization, signifying that rural areas are uniformly obtaining over time a greater share of this budget line. Even the lower and declining coefficients for educational expenditures exhibit an essentially gradual change. Comparing all these trends to the more volatile patterns on the economic side of the budget testifies to the existence of standards more uniformly applied and adhered to across the country for social fiscal outlays. From the previous conclusion regarding the increasing equalization for the principal social expenditures, it is clear that these standards result in lowering fiscal social-cultural, but not economic, differences among the union republics.

The high and stable association between the independent urban variable and funds for socio-cultural expenditures, health and social maintenance, discloses that the level of urbanization is the overwhelming criterion in such funding. Differences in state assistance programs are largely explained by inter-regional demographic and industrial employment patterns. The only kind of expenditures for which both these variables do not account for a major share of the variance is educational funding. Here, inter-

Table 5. Beta Coefficients for Budgetary Expenditures Per Capita Lagged with Percent Population Urban by Soviet Republics, 1958–1970

Year	Total Economy	Industry and Construction	Agriculture and Procurements	Transport and Communications	Housing and Economy	Total Socio-Cultural	Education	Health	Social Maintenance	State Assistance
1958	.819	.716	.618	.533	.462	.763	.630	.745	.662	NA
1959	.767	.756	.501	.575	.558	.751	.666	.709	.677	NA
1960	.826	.877	.533	.532	.560	.785	.675	.673	.634	.117
1961	.853	.870	.587	.606	.585	.787	.649	.658	.655	.102
1962	.824	.821	.627	.496	.585	.772	.637	.576	.664	.120
1963	.753	.877	.511	.511	.684	.741	.534	.575	.681	.111
1964	.745	.823	.506	.463	.665	.765	.575	.534	.678	.113
1965	.827	.927	.575	.530	.672	.816	.673	.559	.673	.070
1966	.646	.587	.659	.506	.700	.847	.698	.641	.682	.079
1967	.508	.594	.596	.493	.673	.854	.537	.638	.677	.080
1968	.471	.700	.428	.493	.554	.803	.573	.628	.692	.047
1969	.561	.687	.587	.432	.550	.797	.621	.604	.683	.023
1970	.343	.720	.589	.551	.741	.727	.537	.455	.633	.010

Sources: Gosudarstvennyi Biudzhet SSSR i Biudzhety Soiuznykh Respublik (Moscow, "Finansy," 1963, 1966, and 1972 editions and from the annual publications of *Nardone Khoziaistvo SSSR* (Moscow, "Statistika").

vening concerns are responsible for the republic differences. The lower coefficients of determination during the late 1960s indicate that the relative advantage in the distribution of educational funds previously held by the urban centers is diminishing in favor of the more rural republics. This is supported by the decline of beta coefficients (Table 5) in the same category, meaning that the amount of per capita monetary surplus above the constant for all regions spent by the urban republics has also diminished over time. The development and extension of primary-secondary education in the more rural areas (Zwick, 1976, pp. 506–08) is thus reflected in these findings.

The beta coefficients in Table 5 are fairly equivalent for the economic and social expenditures (again with the exception of state assistance, where the extremely low betas confirm the fact that most funds in this program are dispersed in agrarian regions). This denotes that for each unit in the independent variable the amount of money spent by the more urban republics in addition to the outlays of the rural regions is essentially the same for both economic and socio-cultural sectors. The important point is that this surplus in urban over rural per capita expenditures accounts practically for all of the small variation in inter-regional social expenditures. In industry and related infrastructures, however, the excess fiscal outlays in the urban areas are responsible for only a small part of the variation in economic expenditures among Soviet republics. In other words, the differences in the economic budget are the result of a greater spending diversity caused by other factors, not primarily related to the urban-rural disparity.

With the exception of the previously described trend in education, the beta coefficient changes over time in the socio-cultural budget are minimal. This means that there is no relative decline in the amount of money urban areas spend above their rural counterparts, despite the trend toward inter-regional equalization in the social services. It is apparent, then, that the increasing parity among Soviet regions for socio-cultural expenditures is closely related to the increasing urbanization of union republics at the lower end of the development scale. Causation cannot be established here; I can only point out that urbanization is associated with larger per-capita expenditures on social activities, and that these developments also bear a close relationship to the increasing equalization of Soviet republics in social expenditures. This interrelationship, of course, is expected and necessary if the creation of better social opportunities is to provide the mobility needed to improve the economic and socio-cultural position of the relatively underdeveloped regions. For whatever reasons, socioeconomic progress is associated with the process of urbanization. By adhering to a standard relying primarily on the levels of urbanization to disburse funding for social benefits, Soviet political and economic leaders have contributed to greater equality among republics. In this sense at least, it can be said that equality is a policy norm in socio-cultural activities. Precisely because other standards are used in the funding of economic sectors, increases in the degree of urbanization have not brought about a comparable reduction in inter-republic differences in the industrial and related sectors.

Is the persistence of inequality in some budgetary categories caused by the failure to develop central equity norms, or by a changing relationship between the center and the subnational units? Having examined health services, Hough has argued that in recent years local forces were "given a greater de facto opportunity to deviate from central norms" (1977, p. 164). In that study, urbanization levels were viewed as the norms utilized by Soviet decision makers to allocate regional resources. Strict adherence to this norm, therefore, results in a very high explained variance for policy outputs, while declining r^2's indicate the increasing influence of regional political actors in decision making or policy implementation. On the basis of his findings, Hough has argued against the notion that the Brezhnev period is one of recentralization, and instead, at least in the health field, finds evidence for dispersion of real authority.

To characterize Soviet trends towards centralization or decentralization completely, one must extend this argument to other policy areas. Unfortunately this cannot be done for all the budgetary categories examined in this study, because the decision-making process in the economic sectors was quite complex and influenced by many factors. The level of urbanization, therefore, cannot be used as the central norm employed by Soviet leaders and planners to decide economic finances.

In the social expenditures, urbanization levels are the primary standards applied for the disbursement of inter-republic funds. Evidence for the degree of regional autonomy can thus be obtained by analyzing the coefficients of determination for the social expenditures in Table 3. The results are mixed. There is a slight trend in the 1957–1970 period toward increasing regional autonomy in the health, education and socio-cultural areas, but the welfare areas of social maintenance and state assistance exhibit constant and increasing levels of centralization respectively. The redistributive nature of the assistance program certainly necessitates a high degree of central control, while social security payments are subject to tight national standards.

On the basis of the evidence, it is difficult to assess whether the Brezhnev period is more decentralized than the 1957–1964 years. Even if a conclusion were possible, its validity would depend on accepting Hough's original assumption that coefficients of determination based on urbanization are adequate measures of autonomy. It is quite possible, however, that the small decrease in explained variance for the health and education budgets is not owning to the more forceful assertion of regional interests but to the intervention of another policy norm. Perhaps the most important point to be made here is that even in those areas where the regional autonomy argument can be made, no negative effect is evident on the pattern of equalization which continued to increase for the socio-cultural budget sectors throughout the 1960s.

Conclusion

The data analyzed here demonstrate that the inter-regional variations among the Soviet republics and the temporal evolution of these differences in the 1957–1970 period cannot be attributed to the application of a particular policy norm for all budgetary expenditures. Single-factor explanations stressing either the Soviet concern with economic efficiency or with political equalization are inadequate for an understanding of Soviet regional development. Instead, we need to take into account the relative impact of the political and economic policy choices in the various aspects of socioeconomic activities, and to consider the additional influence of other factors in the regional distribution of budgetary outlays in the Soviet Union.

The primary distinction in the disbursement of expenditures at the republic level is between the economic and socio-cultural sectors. The evidence indicates that Soviet political and economic leaders appear to be guided by different policy choices in these two areas. In the regional allocation of industrial and related resources, the evidence suggests the use of a multifrontal approach which includes economic growth, security, environmental and political concerns While in the short term this leads to greater inter-republic variations, it may strengthen the national economy so that in the long run a "pull" will occur permitting the development of the backward economic areas. Such a policy of regional economic development accounts for the evident neglect of the equalization goal in the provision of funds in the industrial sector and the necessary supportive infrastructures.

The same approach is not characteristic of the social service sectors. The goal of equalization is more likely to be the operative standard in the inter-republic distribution of public social funds. This is accomplished by reliance on levels of urbanization as criteria for the allocation of socio-cultural resources. Most probably Soviet political decision makers and economic planners look upon urbanization as a process contributing to greater socioeconomic progress and culminating in regional equalization. Indeed, the improvement of educational, health and cultural facilities is likely to result in the creation of new social opportunities in the less developed areas, providing for a social mobility which may in turn improve the socioeconomic status of the people and region in question.

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Turnout and the Vote: The Joke's on the Democrats

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A heavy turnout is commonly believed to favor the Democrats. This study presents theoretical reasoning and empirical evidence that challenge the conventional view. Reasonable assumptions about the behavior of core and peripheral voters lead to the conclusion that the majority party is most likely to suffer when turnout increases, common sense notwithstanding. It also appears that the recent decay of partisan loyalties among voters has eroded the relationship between turnout and the vote.

Ever since the party of FDR became a majority nearly 50 years ago, the idea that a big turnout helps the Democrats has been widely accepted as a self-evident truth. Political life normally defies description by simple and unequivocal general laws, yet few would dispute the columnist Tom Wicker's (1976) suggestion that "a big turnout inevitably helps the majority party." Lurking behind this apparent truism is a complicated and elusive puzzle—a puzzle that is perhaps more difficult to recognize than it is to solve. The greatest obstacle to unraveling the relationship between turnout and the vote is the intuitive feeling that we already understand the problem.

A good way to overcome this obstacle is gathering electoral data to measure how much surges in turnout help the Democrats. The data stand the doctrine on its head with disconcerting regularity. Rather than learning the magnitude of the Democrats' advantage, we are left to wonder why a big turnout does not help them even a little bit. The perplexing scatterplot in Figure 1 is a typical example. In this picture, we have arrayed the share of the vote received by Democrats in recent presidential elections against the percentage of eligible voters who turned out. Even experts on the literature of comparative politics would be hard pressed to name another pair of political variables that are so dismally unrelated.

To see why intuition serves us so poorly here requires a good deal of effort. Specifying how the level of turnout affects the vote is much more challenging than first appearances indicate. The effort is well repaid, however, because the style of reasoning needed to solve this riddle can be used to study the partisan implications of all sorts of electoral change including extensions of the franchise, "electoral disaggregation" (Burnham, 1969), the realignment of support for the parties, and the reform of registration laws. The key to solving any of these problems is a detailed understanding of the process that transforms partisan divisions among eligible voters into the actual vote. Coming to grips with this fundamental issue is the central purpose of the analysis of turnout that follows. Along the way, we shall also learn why there is little reason to expect that a big turnout helps the majority party, why the Republicans have probably benefited more from surges in turnout than the Democrats, at least since the Great Depression, and why the relationship between turnout and the vote has recently disappeared.

A Framework for Analyzing a Two-Party System

The first issue to resolve is the logical relationship between the vote and the level of turnout. While this relationship is straightforward enough, it is by no means simple. Figure 2 shows how the partisan balance among eligible voters is translated into the shares of the vote actually received by each political party (cf. Converse, 1966; Axelrod, 1972). We begin by imagining that the "eligible" electorate can be partitioned into two camps, Democratic and Republican. Depending on one's interests, the eligible electorate may be defined as the set of registered voters or as the entire population of voting age. In any event, B_d and B_r represent the fraction of eligible voters who are partisans

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of each political party. By partisanship we simply mean feelings of identification or attachment that incline a person to vote for one party rather than the other.

The eligible electorate is transformed into the voting electorate by the rates of turnout within each party's base of support. Θ_d and Θ_r represent the fraction of eligible Democrats and Republicans who actually vote. Consequently, $\Theta_d B_d$ represents the size of the Democratic voting bloc, $\Theta_r B_r$ the Republican, and their sum equals the general rate of turnout in the entire electorate (expressed as a fraction of eligible voters):

$$T = \Theta_d B_d + \Theta_r B_r. \qquad (1)$$

Of course, some members of each voting bloc cross party lines in every election. The rates of defection from each camp are denoted by ρ_d and ρ_r. Summing the votes of loyal Democratic partisans and defecting Republicans yields the raw vote for the Democratic candidate:

$$RV_d = (1 - \rho_d)\Theta_d B_d + \rho_r \Theta_r B_r. \qquad (2)$$

The Democrat's share of the vote, V_d, equals his or her raw vote divided by the total turnout, T:

$$V_d = \frac{(1 - \rho_d)\Theta_d B_d + \rho_r \Theta_r B_r}{\Theta_d B_d + \Theta_r B_r}. \qquad (3)$$

It may be verified that $V_d + V_r = 1$.

Equation (3) expresses the Democratic candidate's share of the vote as a function of turnout and the other variables in our model. Let us put it to work first by dissecting the arguments of political pundits who purport to explain why a heavy turnout favors the Democrats. The question is whether we should believe the data in Figure 1 and dismiss the pundits' arguments, or whether the pundits' arguments do make sense, and we have somehow misinterpreted the data.

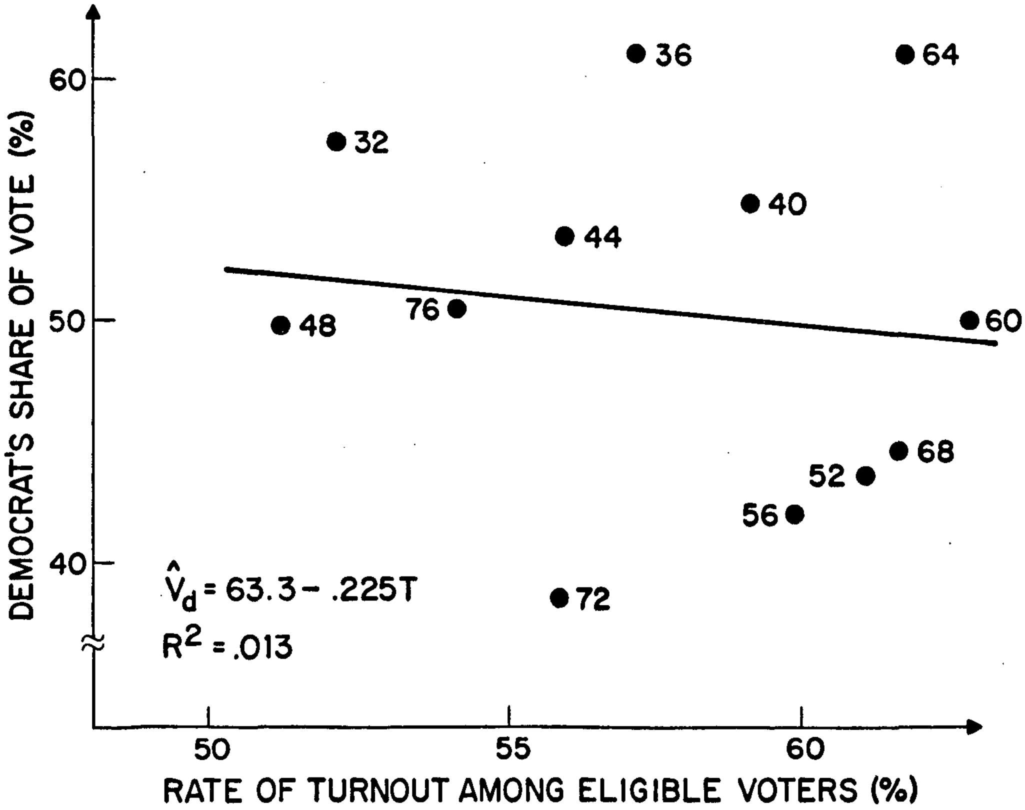


Figure 1. Turnout and the Presidential Vote, 1932–1976

Source: Statistical Abstract of the United States.

Argument I:
This One Misses the Point

The idea that a heavy turnout favors the Democrats became popular during the Great Depression, after a pervasive realignment of party loyalties made the Democrats a majority party.[1] That a heavy turnout favors the majority party certainly seems common-sensical and even trivially self-evident. After all, can it be anything but a boon to the Democrats if most of the eligible voters turn out when a majority of them have Democratic sympathies? Let us examine our model to see whether a majority party necessarily benefits from a heavy turnout. For the moment, we may ignore the complications that arise when defections across party lines are substantial. We can learn all we need to know about this argument by studying the simpler situation where every voter casts a straight party ticket.

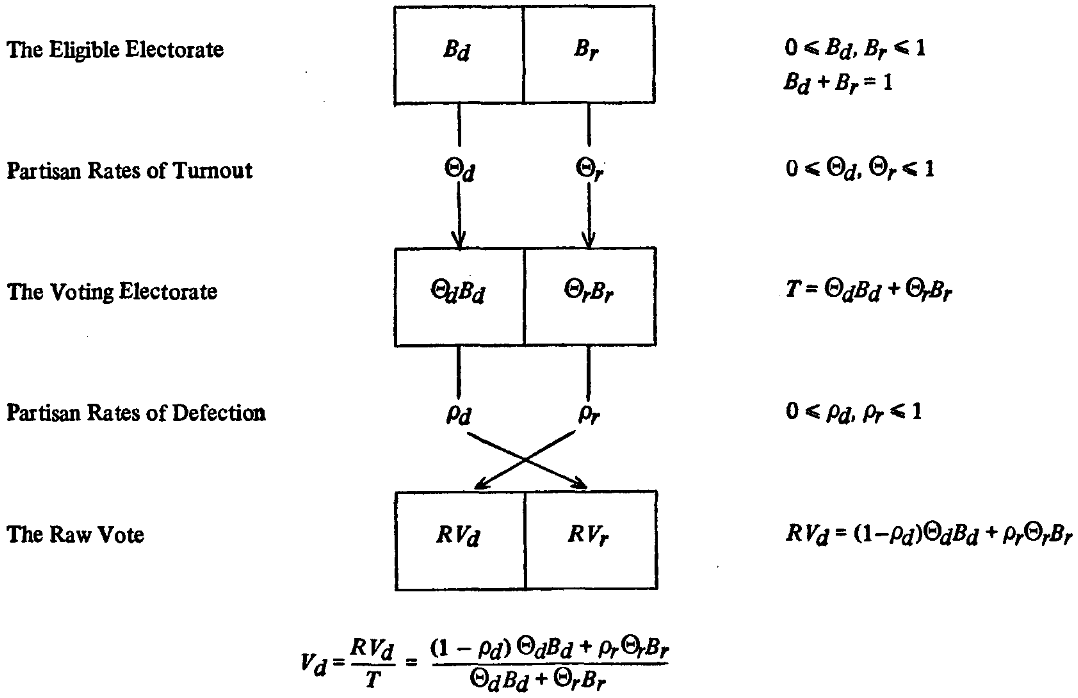
Under these circumstances $\rho_d = \rho_r = 0$ in Equation (3), and the Democrat's share of the vote can be expressed as follows:

$$V_d = \frac{\Theta_d B_d}{\Theta_d B_d + \Theta_r B_r}.$$

The Democrat's share is simply the size of the Democratic voting bloc divided by the total size of the voting electorate. Now suppose that Democrats always turn out at the same rate as Republicans. We then have $\Theta_d = \Theta_r = \Theta$, and the expression for V_d can be simplified still further:

$$V_d = \frac{\Theta B_d}{\Theta(B_d + B_r)} = B_d.$$

The Democrat's share of the vote is completely unaffected by the level of turnout in this situation and in fact equals B_d however


The Eligible Electorate
B_d
B_r
$0 \leq B_d, B_r \leq 1$
$B_d + B_r = 1$
Partisan Rates of Turnout
Θ_d
Θ_r
$0 \leq \Theta_d, \Theta_r \leq 1$
The Voting Electorate
$\Theta_d B_d$
$\Theta_r B_r$
$T = \Theta_d B_d + \Theta_r B_r$
Partisan Rates of Defection
ρ_d
ρ_r
$0 \leq \rho_d, \rho_r \leq 1$
The Raw Vote
RV_d
RV_r
$RV_d = (1-\rho_d)\Theta_d B_d + \rho_r \Theta_r B_r$

$$V_d = \frac{RV_d}{T} = \frac{(1-\rho_d)\Theta_d B_d + \rho_r \Theta_r B_r}{\Theta_d B_d + \Theta_r B_r}$$

Source: Compiled by the author.

Figure 2. A Model of the Electoral Process

many voters participate in the election! There is no disputing the claim that the Democrats do well when turnout is high and B_d is greater than B_r. The point is that they do just as well when turnout is low. In other words, showing that the Democrats win when turnout is heavy does not justify the inference that a heavy turnout helps the Democrats. The common-sense argument confuses the notion of winning with the notion of helping. Information about the division of partisan loyalties in the population is simply not sufficient to support inferences about the relationship between turnout and the level of electoral support for each party. Evidently, a plausible story must focus instead on *differential rates of turnout* within the two partisan camps.

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[1] Duane Lockard and Harold Gosnell concur in their recollections about the origins of this popular belief.

Argument II: An Argument that Makes More Sense

Consider next a slightly more complicated argument that focuses upon the propensity to vote among each party's supporters rather than upon the majority status of the Democrats. The keystone of the argument is that Democratic partisans are more fickle in turning out than their Republican counterparts. This contention is usually buttressed by observations about demographic patterns of partisan support. Democratic identifiers are thought to be poorer, less educated, more "ethnic," and more urban than their Republican opposite numbers, and therefore the more "marginal" or "peripheral" voters. (See Berelson et al., 1954; Campbell et al., 1954; Milbrath and Goel, 1977.)

Peripheral voters are conventionally defined as people whose interest in political affairs is passing (Campbell, 1960). They do not have strong feelings of attachment to either of the political parties, and make decisions about voting largely in response to the "level of excitement" associated with particular elections.[2] During presidential campaigns and other intense electoral contests, news and propaganda rain down on the voters in such prodigious quantities as to reach even the outermost periphery of the electorate, prompting more than the usual number of politically uninterested citizens to vote. Less noisy campaigns escape their notice and create little impetus to turn out. One result is the well-known ratchet pattern in turnout figures. The level of turnout is decidedly lower in midterm election years than in the more "exciting" presidential years. The box and whisker plots (see Tukey, 1977) in Figure 3 capture this effect very clearly. They describe the distribution of statewide rates of turnout in recent congressional elections.

While Democratic partisans are considered more likely to have characteristics associated with peripherality, Republican identifiers are thought more likely to be "core" voters. Core voters are more interested in politics, have stronger partisan attachments, and manifest a more robust "sense of duty" about voting. They are the party faithful who turn out regularly to vote for everyone from presidential contenders to local aldermen, regardless of who heads the ticket.

It follows that the Democrats turn out less consistently than the Republicans. If the demographic tendencies noted by the pundits are pronounced, the rate of turnout among Republicans should change little from election to election while the rate among Democrats fluctuates substantially with the level of excitement. Consequently, the Democrats should profit when turnout surges and more peripheral voters are drawn into the voting electorate, since a higher proportion of their supporters are cast from the peripheral mold.

Evaluating Argument II

Does Argument II really imply that a heavy turnout favors the Democrats? To find out, we must elaborate the model a bit further. Let

C_d, C_r	be defined as the fraction of each party's base camp composed of "core voters," where $0 \leqslant C_d, C_r \leqslant 1$;
P_d, P_r	represent the fraction of each party's base composed of "peripheral" voters, where $0 \leqslant P_d, P_r \leqslant 1$;
$\frac{P_d}{P_r}$	be called the "peripherality ratio." Argument II assumes the peripherality ratio to be greater than unity;
Θ_c	be the rate of turnout among core voters where $0 \leqslant \Theta_c \leqslant 1$;
Θ_p	be the rate of turnout among peripheral voters where $0 \leqslant \Theta_p \leqslant 1$.

We assume that each member of the eligible electorate can be classified as either a core or peripheral voter. Hence $C_d + P_d = 1 = C_r + P_r$.

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[2] We shall not attempt to decide here whether the "level of excitement" is largely a matter of how much brouhaha surrounds a campaign or, alternatively, a reflection of how important the political stakes seem to be. The evidence is far from clear, and, in any event, neither explanation precludes the other.

Core voters turn out regularly while peripheral voters are noted for their fickle political involvement. We shall assume then that core voters, as a group, participate at a constant rate from one election to the next while the rate of turnout among peripheral voters varies with the level of excitement. The quantity Θ_c is therefore a constant (presumably close to but not necessarily equal to 1), while Θ_p varies from election to election. The rates of turnout within each partisan camp, Θ_d and Θ_r, reflect the mix of core and peripheral types attracted to each party and the level of participation by each kind of voter:

$$\Theta_d = C_d\Theta_c + P_d\Theta_p \tag{4}$$

$$\Theta_r = C_r\Theta_c + P_r\Theta_p . \tag{5}$$

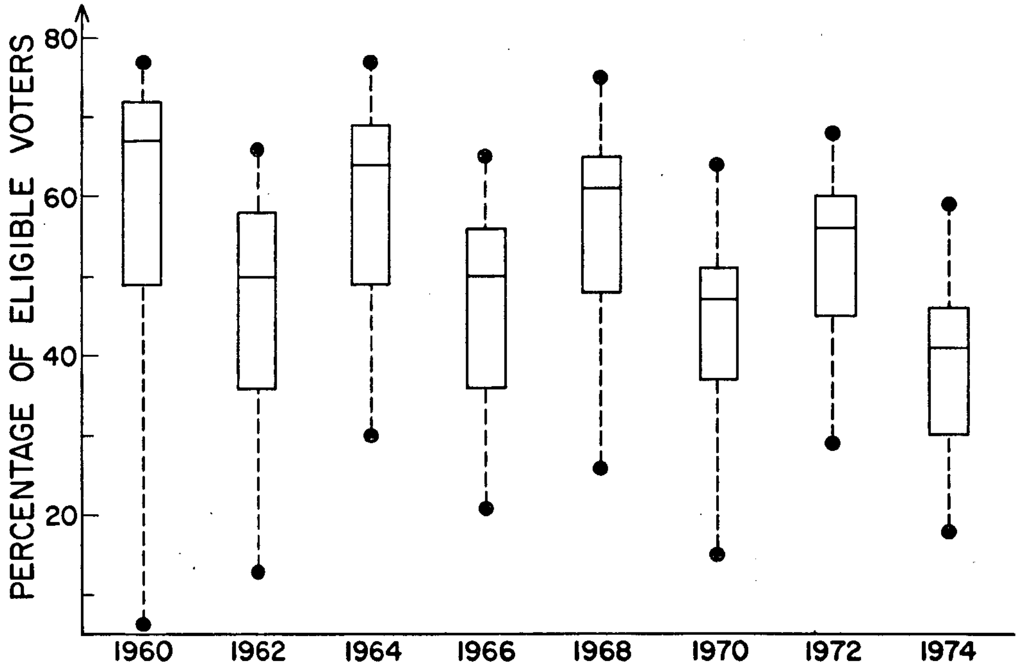
An important consequence of these assumptions is that the partisan rates of turnout, Θ_d and Θ_r, are linearly related to each other.[3] It is easily shown that

$$\Theta_d = M\Theta_r - K \tag{6}$$

where $M = P_d/P_r$ is the peripherality ratio and

$$K = \Theta_c(M - 1). \tag{7}$$

Once the blend of core and peripheral voters in each camp is fixed, M and K are simply



Source: Statistical Abstract of the United States.

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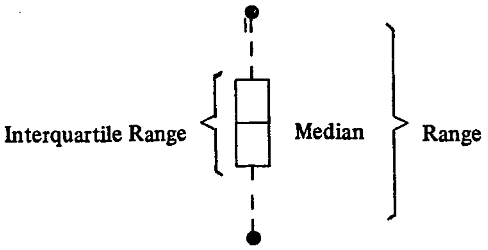

Figure 3. Statewide Rates of Turnout in Recent Congressional Elections

constants. Melding equations (6) and (3) yields a new and useful expression for the Democrat's share of the vote:

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[3] This relationship is not unique to the world of Argument II. See Appendix 1 for a proof of the proposition.

$$V_d = \frac{(1 - \rho_d)(M\Theta_r - K)B_d + \rho_r\Theta_r B_r}{T} \qquad (8)$$

where

$$T = \Theta_r(MB_d + B_r) - KB_d.$$

Regarding everything but Θ_r (and implicitly Θ_d and T) as fixed, we can differentiate this equation with respect to Θ_r to learn how marginal changes in turnout affect the vote. It can be verified that

$$\frac{\partial V_d}{\partial \Theta_r} = \frac{(1 - \rho_d - \rho_r)KB_dB_r}{T^2}$$

and thus[4]

$$\frac{\partial V_d}{\partial T} = \frac{(1 - \rho_d - \rho_r)KB_dB_r}{T^2(MB_d + B_r)}. \qquad (9)$$

It is easy to see that this partial derivative is (virtually) always positive, and consequently that the Democrat's share of the vote increases whenever turnout does. B_d, B_r, T, and M are always positive numbers in the world of Argument II. The expression $(1 - \rho_d - \rho_r)$ is positive as long as defections in each camp remain below 50 percent, as they virtually always do. It follows that $\frac{\partial V_d}{\partial T}$ is positive whenever K is. Since $K = \Theta_c(M-1)$, and Θ_c is always positive, we see at last that the peripherality ratio, M, is the pivotal issue. When M is greater than one, as Argument II assumes, K is positive and a big turnout does help the Democrats. As the proportions of peripheral voters in the two partisan camps converge, M approaches one, K goes to zero, and the relationship between turnout and the vote disappears.[5]

Unlike Argument I, Argument II makes logical sense. Democratic candidates benefit from a heavy turnout if their party relies more on peripheral supporters than do Republicans. Stubborn adherents of Argument I should note that a positive relationship between turnout and the vote always exists in the world of Argument II, regardless of how eligible voters divide their loyalties. We turn our attention next to the form of this relationship. When is it most pronounced and when is it most attenuated, and how is it affected by changes in the political environment?

The Relationship According to Argument II

Equation (9) indicates that the relationship between the rate of turnout and the Democrat's share of the vote is rather complicated, changing whenever the contours of the electoral situation change. Figure 4 graphs this distinctly non-linear relationship for simulated values of B_d, M, and Θ_c. Several rules about the behavior of these graphs can be derived from the higher-order derivatives of Equation (8). (See Appendix 2 for the mathematical details.)

Rule 1: As the general rate of turnout, T, increases, the relationship between turnout and the Democrat's share of the vote becomes less pronounced.

Rule 2: As the peripherality ratio grows larger, the relationship between turnout and the vote becomes more pronounced.

Rule 3: As defections in either or both camps increase, the relationship becomes weaker, disappearing entirely when voting becomes totally nonpartisan, that is, when $\rho_d = \rho_r = ½$.

Rule 4: As the partisan division of the eligible voters becomes extremely skewed, the relationship grows fainter.

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[4] When everything but Θ_r is held constant, T is a simple linear function of that variable. $\frac{\partial V_d}{\partial T}$ can be derived from $\frac{\partial V_d}{\partial \Theta_r}$ by applying the chain rule for partial derivatives:

$$\frac{\partial V_d}{\partial \Theta_r} = \frac{\partial V_d}{\partial T}\frac{\partial T}{\partial \Theta_r}$$

thus

$$\frac{\partial V_d}{\partial T} = \frac{\partial V_d}{\partial \Theta_r} \Bigg/ \frac{\partial T}{\partial \Theta_r} \qquad \text{where } \frac{\partial T}{\partial \Theta_r} = (MB_d + B_r).$$

[5] The reader may inspect Equation (6) to verify that $\Theta_d = \Theta_r$ when $M = 1$. We have already seen that no relationship exists between turnout and the vote when partisans of each camp participate at identical rates.

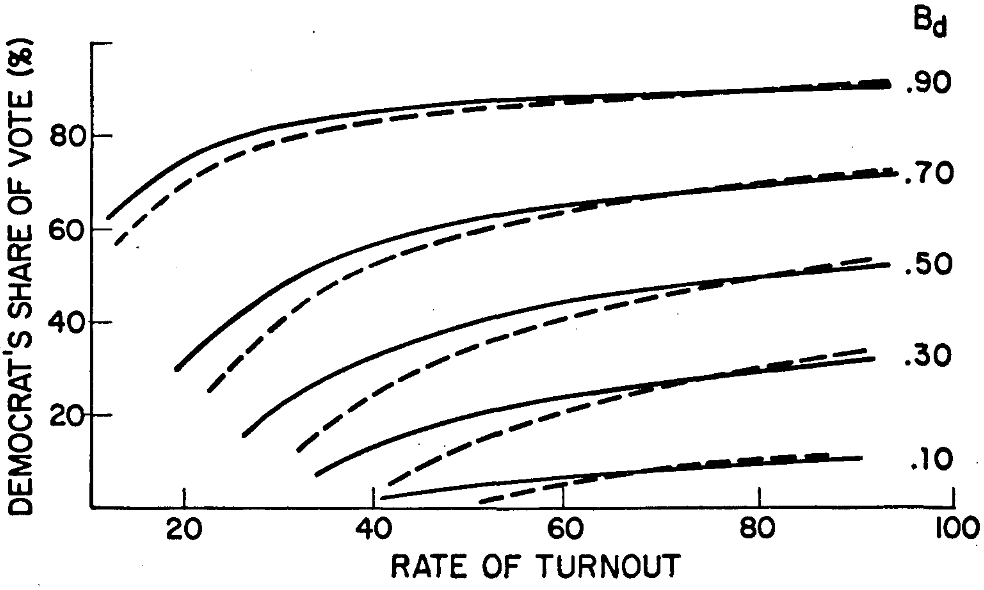
An Analysis of Voting Data

Having considered the logical issues raised by the pundits' arguments, we shall now turn to electoral data to see whether a careful analysis might reveal a relationship between turnout and the vote. Rules 2 and 3 suggest the critical issues. First, do the Democrats rely on peripheral support sufficiently more than the Republicans to create a pronounced relationship? Second, are defections across party lines sufficiently low that a relationship will not be washed away if it might otherwise exist?

To study the problem, we have collected data on congressional voting during six election years spanning a period of three decades from the late 1930s through the middle 1960s. Our sample of congressional districts is drawn from large and small, northern and southern states. Included are districts from California, Oregon, Pennsylvania, West Virginia, Florida, Maryland, and New York. For each congressional election, we have tabulated the proportion of the major party vote going to the Democratic candidate (V_d), the level of turnout, expressed as a proportion of registered voters (T), and the proportion of registered voters affiliated with each party (B_d,B_r). (See Appendix 3 for further details.)

Fitting lines to bivariate scatterplots of the vote and turnout data (as in Figure 1) is a congenial analytical approach both in its directness and ease of interpretation. Two problems need to be resolved before we adopt it, however. The first is that scatterplots of this kind are unlikely to be informative until statistical controls are placed on the distribution of partisan loyalties in each district. Figure 4 illustrates how the relationship between the vote and turnout is displaced and transformed whenever the partisan balance in the eligible electorate changes. To mitigate this difficulty, we have collected information on enough individual elections (over 300) that the data may be partitioned into relatively sizable subsamples which are homogeneous with respect to the percentages registered for each party.

The second difficulty involves the nonlinearity of the relationship between turnout and the vote, even after the partisanship of the electorate is held constant. Again Figure 4 illustrates the problem. Fitting straight lines to crooked relationships opens a Pandora's Box of statistical miseries. Experience has shown that



Source: Compiled by the author.

Values of simulated parameters:	Turnout	Defections	Peripherality Ratio
	$0 < \Theta_p < 1$	$\rho_d = 0 = \rho_r$	M = 3 ------
	$\Theta_c = .80$		M = 2 ———

Figure 4. Turnout and the Vote in the World of Argument II

unbending crooked data to fit linear techniques is much easier than bending linear techniques to fit crooked data, and this is the tack we shall follow (see Tukey, 1977). Given the premises of Argument II, the relationship between the Democrat's share of the vote and the negative inverse of turnout ($-1/T$) is linear when other aspects of the electoral situation remain fixed.[6] This monotonic transformation preserves all the information in the data about how the vote is affected by changes in turnout. If the relationship between the level of support for Democrats and turnout is positive, so too is the relationship between the vote and the negative inverse of turnout. The transformation merely allows us to describe the relationship in a particularly simple way—as the equation of a straight line.

Table 1 contains summary statistics from regression analyses of the congressional data. The data are partitioned by year and the percentage of registered voters affiliated with each party. Among the 36 subsamples, 27 contain enough cases to make the fitting of lines meaningful. In each subsample, a regression line with slope b and intercept a summarizes the relationship between the proportion of the vote received by the Democrats (V_d) and the negative inverse of turnout ($-1/T$). Below each of the slope estimates is the estimated standard error of the coefficient. If the Democratic vote tends to increase with the level of turnout, the fitted line has a positive slope. A negative slope indicates the reverse relationship. The value of the slope measures the change in the Democratic share of the vote associated with a unit change in the variable measuring turnout. Note that the negative inverse of turnout increases by one unit when the actual level of turnout changes from 1/4 to 1/3, 1/3 to 1/2, or 1/2 to 1. Given the variation in turnout observed in the sample, a reasonable interpretation of an estimated slope of .149 is that the Democratic vote improves by 14.9 percent, on the average, as turnout increases from 50 to 100 percent.

While the estimates are often statistically unstable in individual samples, several clear patterns emerge in the table as a whole. The election years before 1962 yield estimated slopes which are usually substantial in magnitude, although their signs vacillate curiously. The R^2 statistics in these samples typically are low but not vanishingly small, indicating that the relationship between turnout and the vote is faint but discernible. This result is not surprising since the short-term partisan tides that develop in particular elections can easily overwhelm the subtler effects of turnout. The slopes from 1962 and 1966 are generally very close to zero, and the corresponding R^2 values vanish to nothingness.

Argument III:
The Case of the Switching Signs

The earlier data in the sample are uncorrupted by the soaring defections that beset elections during the 1960s and in them we discover the most intriguing aspect of all our findings—the vacillating signs of the fitted slopes: The instability of the signs does not seem at all haphazard. Rather they tend to be positive in districts where the Republicans are a majority and negative when the Democrats hold the edge in registration. For all its plausibility, Argument II simply cannot account for this feature of the data.

Converse (1966) has made one suggestion that might explain our results. By simulating data with his normal vote model, Converse found that declining turnout may strengthen the electoral position of a majority party. His argument turns on the behavior of "independent" voters. Members of this group are the first to leave the voting electorate when the level of excitement is low, and the most likely to cast split tickets when they do vote. When turnout falls, the loss of the independent vote often deprives the minority party of a much larger proportion of its support than it does the majority, since independents tend to divide their votes equally.

From our analytical perspective, this argument hinges on the notion that rates of defection vary with the level of turnout. As more peripheral voters are drawn to the polls, the aggregate rates of defection in each party increase. This possibility has been overlooked in

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[6] Define $I = -1/T$. Then by the chain rule

$$\frac{\partial V_d}{\partial T} = \frac{\partial V_d}{\partial I}\frac{dI}{dT} \text{ and } \frac{\partial V_d}{\partial I} = \frac{\partial V_d}{\partial T} \Big/ \frac{dI}{dT}$$

when everything but rates of participation are held constant. Since

$$\frac{dI}{dT} = 1/T^2, \frac{\partial V_d}{\partial I} = \frac{(1-\rho_d-\rho_r)KB_dB_r}{(MB_d+B_r)}, \text{ and}$$

the value of this partial derivative does not vary with the level of turnout. Thus the relationship between the vote and the negative inverse of turnout is linear when all other variables are held constant. Let us note in advance that this argument also applies to Argument III.

Argument II, so let us adapt our model to consider it more closely. Let ρ_p represent the rate of defection among peripheral voters and ρ_c be the rate of defection among core voters. The rates of defection in each voting bloc can then be expressed as follows:

$$\rho_d = \frac{\rho_c \Theta_c C_d + \rho_p \Theta_p P_d}{\Theta_d} \tag{10}$$

and

$$\rho_r = \frac{\rho_c \Theta_c C_r + \rho_p \Theta_p P_r}{\Theta_r}. \tag{11}$$

The weaker partisanship of peripheral voters makes them more likely than core regulars to cross party lines. Accordingly, let $\rho_p = \rho_c + \delta$ where $\delta > 0$ and then by substitution

Table 1. Regression Analysis of Congressional Voting

$$V_d = a + b(-1/T)$$

Year	$B_d < 30$	$30 \leqslant B_d < 40$	$40 \leqslant B_d < 50$	$50 \leqslant B_d < 60$	$60 \leqslant B_d < 70$	$B_d \geqslant 70$
1938	b = .169 (.146) a = .516 R^2 = .183 N = 8	*	*	b = –.083 (.043) a = .398 R^2 = .237 N = 14	b = –.182 (.053) a = .303 R^2 = .699 N = 7	b = –.539 (.326) a = .067 R^2 = .406 N = 6
1946	N = 4	b = .153 (.109) a = .611 R^2 = .131 N = 15	b = .417 (.205) a = 1.08 R^2 = .579 N = 5	b = .034 (.087) a = .514 R^2 = .016 N = 11	b = –.100 (.088) a = .329 R^2 = .204 N = 7	N = 3
1950	b = .700 (.287) a = 1.37 R^2 = .459 N = 9	b = .481 (.423) a = 1.14 R^2 = .156 N = 9	b = .491 (.406) a = 1.13 R^2 = .267 N = 6	b = –.245 (.113) a = .133 R^2 = .280 N = 14	b = –.168 (.082) a = .290 R^2 = .343 N = 10	N = 2
1954	b = .626 (.972) a = 1.29 R^2 = .094 N = 6	b = .943 (.458) a = 1.75 R^2 = .320 N = 11	b = .663 (.332) a = 1.41 R^2 = .399 N = 8	b = .064 (.092) a = .565 R^2 = .027 N = 19	b = –.040 (.049) a = .543 R^2 = .070 N = 11	b = .125 (.169) a = .863 R^2 = .154 N = 5
1962	N = 2	N = 3	b = –1.74 (1.32) a = –1.85 R^2 = .179 N = 10	b = –.204 (.153) a = .204 R^2 = .067 N = 27	b = .204 (.155) a = .859 R^2 = .062 N = 28	b = –.021 (.101 a = .620 R^2 = .004 N = 13
1966	N = 4	N = 1	b = –.013 (.097) a = .366 R^2 = .002 N = 11	b = –.036 (.129) a = .375 R^2 = .004 N = 21	b = .038 (.093) a = .632 R^2 = .007 N = 28	b = –.002 (.097) a = .689 R^2 = .000 N = 7

Sources: California: *Roster of Federal, State, County, and City Officials,* and *Statement of the Vote* (Secretary of State, Sacramento); Florida: *Report of the Secretary of State* (Secretary of State, Tallahassee); Maryland: *The Maryland Manual* (Hall of Records Commission, Annapolis); New York: *New York State Legislative Manual* (Department of State, Albany); Oregon: *The Oregon Blue Book* (Secretary of State, Salem); Pennsylvania: *The Pennsylvania Manual* (Department of Property and Supplies, Harrisburg); West Virginia: *The West Virginia Blue Book* (Clerk of the Senate, Charleston).

*Fit dominated by single outlier. Results ambiguous.

$$\rho_d = \rho_c + \frac{\delta \Theta_p P_d}{\Theta_d} \tag{12}$$

and

$$\rho_r = \rho_c + \frac{\delta \Theta_p P_r}{\Theta_r}. \tag{13}$$

Under these assumptions it is easy to show, and entirely obvious, that the rates of defection in each party increase as the level of turnout among peripheral voters increases.[7] Our model now captures the essence of Converse's argument, and we can study its implications by substituting the new expressions for ρ_d and ρ_r into Equation (8). The problem is to see how the share of the vote received by the Democrats changes with the rate of participation among peripheral voters. Differentiating with respect to Θ_p yields the equations set forth in Figure 5.

Equation (14) appears intimidating but it is quite a friendly beast. We can begin to unravel it by imagining for a moment that there is no difference in the propensity to defect among core and peripheral voters. Then $\delta = 0$ and the long string of terms on the right of the numerator disappears, leaving behind:

$$\frac{\partial V_d}{\partial T} = \frac{(1-\rho-\rho)KB_dB_r}{T^2(MB_d + B_r)}.$$

We are now back on familiar ground. This expression is nearly identical to the one derived from Argument II,[8] differing only in that ρ_d and ρ_r are constrained to be equal when we set $\delta = 0$.

It is not true, however, that Converse's argument is merely a corollary of earlier reasoning. To see why not, let us weaken the central premise of Argument II by assuming that the two parties attract equal proportions of peripheral supporters, while letting δ become positive once again. This is accomplished by setting $P_d = P_r = P$, $C_d = C_r = C$, and $K = 0$. Now the terms on the left of the numerator in Equation (14) disappear, leaving

$$\frac{\partial V_d}{\partial T} = \frac{\delta C \Theta_c (B_r - B_d)}{T^2}. \tag{15}$$

The sign of the relationship now depends entirely on the relative size of each party's base in the eligible electorate. When the Republicans attract the greater following, a big turnout helps the Democrats. When the Democrats are the majority party, the Republicans benefit from a heavy turnout.

Converse has thus identified another political mechanism that influences the relationship between turnout and the vote. The process he describes can generate a connection between the two variables even if the basic assumption of Argument II is false (that is, if $P_d = P_r$). By the same token, it is not necessary to assume that peripheral voters are especially prone to defection (as Argument III does) to assert the validity of Argument II. In this sense, the two processes are independent. The interesting thing to notice is that they may sometimes pull in opposite directions. The effects described in Argument III force us to revise substantially the conclusions drawn earlier from Argument II. To understand why and how, consider Equation (14) once again.

Let us first investigate districts that are heavily Republican. In these areas, the absolute size of the Republican periphery is likely to be greater than the Democratic (i.e., $P_rB_r > P_dB_d$), even though a larger proportion of the Democratic partisans may be peripheral voters. Equation (14) tells us that a big turnout helps

$$\frac{\partial V_d}{\partial \Theta_p} = \frac{(1-2\rho_c)KB_dB_rP_r + \delta(P_rB_r - P_dB_d)\Theta_c(C_dB_d + C_rB_r)}{T^2}$$

$$\frac{\partial V_d}{\partial T} = \frac{(1-2\rho_c)KB_dB_rP_r + \delta(P_rB_r - P_dB_d)\Theta_c(C_dB_d + C_rB_r)}{T^2(MB_d + B_r)P_r}$$

Source: Compiled by the author.

Figure 5. Equation (14)

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[7] It may be verified that

$$\frac{\partial \rho_d}{\partial \Theta_p} = \frac{\delta P_d \Theta_c C_d}{\Theta_d^2} > 0$$

and

$$\frac{\partial \rho_r}{\partial \Theta_p} = \frac{\delta P_r \Theta_c C_r}{\Theta_r^2} > 0.$$

[8] Compare Equation (9).

Democrats in these districts if either argument is valid—that is, if either K or δ is positive. If peripheral voters are especially likely to defect ($\delta>0$), the benefits to the Democrats of a large turnout are even greater than the reasoning of Argument II leads us to suppose.

The situation is very different in predominantly Democratic districts, however. Here we have $P_rB_r < P_dB_d$, and the right-hand side of the numerator in Equation (14) becomes negative whenever $\delta > 0$. Heavy peripheral defection tends to reverse the positive relationship between turnout and the Democratic share of the vote that we expect from Argument II. The larger δ becomes, the more drastically $\frac{\partial V_d}{\partial T}$ is pulled in the negative direction. Eventually its sign changes and a big turnout actually helps the minority Republicans. The mechanisms described in Arguments II and III therefore reinforce each other in predominantly Republican districts but pull against each other in Democratic strongholds.

The data in Table 1 suggest that both arguments were valid before 1962. Certainly we need Argument III to explain the negative slopes in the Democratic districts. But Argument III cannot account for everything on its own. If Argument II were invalid, it would follow from Equation (15) that the magnitude of the slope estimates should not differ substantially in districts where the partisan balance is equally skewed (i.e., where $|B_r - B_d|$ is equal). In fact, the magnitudes of the estimated slopes in Republican districts are much greater than they are in the corresponding Democratic districts. This pattern may be attributed to the simultaneous operation of both dynamics. In Republican districts they reinforce each other, while in Democratic districts their effects cancel out.

Life Without a Core

The most recent data in our sample pose a final puzzle. Without fanfare or forewarning, the relationships that we have discerned in the earlier data simply disappeared in the 1960s. Even the most casual survey of the voting literature makes it clear that the sixties and seventies were years of unprecedented electoral turmoil. In simpler times, habitual party-line balloting was common, lending stability to the aggregate vote. This stability has now evaporated in a rising tide of defections. At the same time, rates of turnout have plummeted as seen in Figure 3. The problem is to find an explanation that is at once compatible with these facts and enlightening about what happened to the connection between turnout and the vote.

The new evidence seems at first to contradict the reasoning of Argument III which rests on the idea that defections increase as turnout does. We must be cautious about abandoning the ship too soon, however. Surely it is folly to accept the superficial idea that the core is suddenly more prone to defection than the periphery in order to explain why defections have increased as turnout has fallen. On the contrary, Argument III still makes perfectly good sense. The root of the problem lies elsewhere in the radical transformations that have reshaped the electorate. All the evidence suggests that the peripheral sectors in both parties have swollen while the loyal cores have withered (see DeVries and Tarrance, 1972; Nie et al., 1976; Miller and Levitin, 1976; and Converse, 1976).

Consider first the impact of this trend on rates of defection and the level of turnout. Accepting the premise that peripheral voters defect more than core voters, we may differentiate Equations (12) and (13) with respect to P_d and P_r for a measure of how rates of defection change as the periphery of each party grows larger. We find

$$\frac{\partial \rho_d}{\partial P_d} = \frac{\delta \Theta_p \Theta_c C_d}{\Theta_d{}^2} > 0$$

and

$$\frac{\partial \rho_r}{\partial P_r} = \frac{\delta \Theta_p \Theta_c C_r}{\Theta_r{}^2} > 0.$$

Thus the general rates of defection increase as the ranks of the periphery multiply.

At the same time participation declines. The general rate of turnout can be expressed as follows:

$$T = \Theta_c C_s + \Theta_p P_s$$

where

$$C_s = C_d B_d + C_r B_r$$

and

$$P_s = P_d B_d + P_r B_r.$$

Here C_s and P_s measure the size of the core and peripheral sectors in the electorate as a whole. Since $C_s + P_s = 1$,

$$T = \Theta_c + (\Theta_p - \Theta_c) P_s$$

and

$$\frac{\partial T}{\partial P_s} = (\Theta_p - \Theta_c).$$

If core voters turn out more heavily than peripheral voters, and they surely do, then the general rate of turnout decreases as the peripheral sector grows. The withering of the core is thus sufficient to explain the predominant trends in recent elections.

What then is the fate of the connection between turnout and the vote in the new political climate? Perhaps the easiest way to appreciate the consequences of a shrinking core is to consider the degenerate case where the core disappears entirely. We can see what happens by setting the size of the core equal to zero in Equation (16) (see Figure 6). Remember that the size of the periphery goes to one as the size of the core goes to zero. As this happens, the expression for the Democratic share of the vote becomes radically more simple:

$$V_d = (1 - \rho_p)B_d + \rho_p B_r.$$

The vote is no longer affected by fluctuations in turnout because the mechanisms described in Arguments II and III hinge on differences in the behavior of core and peripheral voters. In an electorate without a core, the vote is governed solely by the aimless defections of the now universal periphery. The forces that wither the core are likely to weaken partisanship in the periphery as well. Voters of the new era disregard party labels, responding instead to personalities and issues over which neither party maintains monopoly control. From a long-term perspective the aggregate rate of defection, ρ_p, approaches one-half as voters become equally likely to support candidates in either party. Since

$$V_d = \rho_p + (1 - 2\rho_p)B_d,$$

the partisan division of the vote simply hovers around 50 percent. In other words, the association between the vote and the partisan alignment of the electorate disappears.

A world without a core is a world of confusion. Formerly stable relationships are lost in a sea of random defection, and the electoral analyst is cast adrift without the anchorage that habitual partisan voting once provided. Subtle relationships like the one between turnout and the vote are the first casualties. As the process of "disaggregation" continues, even the gross relationships disappear. In the end, the concept of partisanship no longer makes sense as a description of people's behavior or as the foundation of a conceptual paradigm. The problem is to find new conceptual lenses that can resolve structure and meaning in data which appear random from old perspectives. This puzzle is far more challenging than the one we have studied here.

Summary

Does a big turnout help the Democrats? Let us summarize our complicated answer to this simple question by reviewing the arguments presented in the text.

Argument I incorrectly asserts that a heavy turnout necessarily favors the majority party. It mistakenly assumes that a small voting electorate must have a different partisan coloration than a large one. If Democrats and Republicans always turn out at equal rates, however, this is not the case. The partisan balance in the eligible electorate does influence the relationship between turnout and the vote, but in ways that are far more subtle than this simple intuitive argument suggests.

Argument II assumes that the Democratic party attracts a higher proportion of peripheral voters—people whose rate of turnout varies with the level of political excitement. When the

$$V_d = \frac{\left(1 - \frac{\rho_c\Theta_c C_d + \rho_p\Theta_p P_d}{\Theta_c C_d + \Theta_p P_d}\right)(\Theta_c C_d + \Theta_p P_d)B_d + (\rho_c\Theta_c C_r + \rho_p\Theta_p P_r)B_r}{\Theta_c C_s + \Theta_p P_s}.$$

Source: Compiled by the author.

Figure 6. Equation (16)

campaign season is boring and uneventful, the Democrats suffer because a larger fraction of their supporters stay home on election day. Exciting campaigns draw peripheral voters to the polls, to the Democrats' advantage. Our analysis shows that this reasoning is quite sturdy as long as defection rates remain low. The essential flaw of Argument II is that it treats the level of turnout and rates of defection as independent quantities.

Argument III recognizes that peripheral voters are just as fickle inside the voting booth as they are about getting to it. These voters cross party lines more frequently than core regulars because their partisan attachments are weaker. As a result, the rates of defection in each camp increase with the level of turnout. The minority party benefits if it can capture the same fraction of defectors from the majority bloc that it loses to them. The larger this fraction becomes, the better for the minority. For this reason, campaigners for the minority party should celebrate when the fickle periphery turns out in force.

The general message of the congressional data is that a big turnout has helped the minority, at least until recently. The parties have not been affected symmetrically, however, because of the mechanism described by Argument II. The Democrats benefit more from a big turnout in Republican districts than the Republicans benefit in Democratic districts. Of course, the Democrats are the majority in most districts. Lately, the picture has changed because the cores have withered in both parties. Arguments II and III hinge upon differences in the behavior of core and peripheral voters. When the core disappears, so does the relationship between turnout and the vote.

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Appendix 1.

PROPOSITION: $\Theta_d = M\Theta_r - K$ where $M = P_d/P_r$ and $K = \Theta_c(M-1)$.

PROOF: We first manipulate the expression for K:

$$\begin{aligned} K &= \Theta_c(M-1) \\ &= \Theta_c P_d/P_r - \Theta_c + (\Theta_c P_d - \Theta_c P_d) \\ &= \frac{\Theta_c P_d(1-P_r)}{P_r} - \Theta_c(1-P_d) \\ &= \frac{\Theta_c(P_d C_r - P_r C_d)}{P_r}. \end{aligned}$$

Thus
$$M\Theta_r - K = \frac{P_d(C_r\Theta_c + P_r\Theta_p)}{P_r} - \frac{\Theta_c(P_dC_r - P_rC_d)}{P_r}$$

$$= \frac{P_dC_r\Theta_c}{P_r} + P_d\Theta_p - \frac{P_dC_r\Theta_c}{P_r} + \Theta_cC_d$$

$$= P_d\Theta_p + C_d\Theta_c = \Theta_d.$$

Appendix 2.

Rule 1 says that $\frac{\partial^2 V_d}{\partial T^2} < 0.$

The reader may confirm that

$$\frac{\partial^2 V_d}{\partial T^2} = -2\left[\frac{(1-\rho_d-\rho_r)KB_dB_r}{T^3(MB_d+B_r)}\right].$$

If $M > 1$, K remains positive and the expression in brackets is positive. Therefore the second derivative is negative, and the relationship between the Democrat's share of the vote and turnout diminishes in strength as turnout increases.

Rule 2 says that $\frac{\partial^2 V_d}{\partial M \partial T} > 0.$

The intrepid may verify that

$$\frac{\partial^2 V_d}{\partial M \partial T} = \frac{(1-\rho_d-\rho_r)\Theta_cB_dB_r[T-2(M-1)(\Theta_r-\Theta_c)B_d(MB_d+B_r)]}{T^3(MB_d+B_r)^2}.$$

The quantity in brackets must be positive if the whole expression is to be positive as the rule states. This follows immediately if we can show that $\Theta_c > \Theta_r$. We have already seen that

$$\Theta_r = C_r\Theta_c + P_r\Theta_p$$

$$= (1-P_r)\Theta_c + P_r\Theta_p$$

$$= \Theta_c + P_r(\Theta_p - \Theta_c).$$

If some Republicans are peripheral voters (i.e., $P_r > 0$), then $\Theta_c > \Theta_r$ whenever $\Theta_c > \Theta_p$. Thus the relationship between turnout and the vote grows stronger as the peripherality ratio increases provided that (1) $\Theta_c > \Theta_p$—core voters turn out at a higher rate than peripheral voters; and (2) $P_r > 0$—some Republicans are peripheral voters. These requirements are very weak so Rule 2 is virtually always valid.

Rule 3 says that $\frac{\partial^2 V_d}{\partial \rho_d \partial T} < 0$ and $\frac{\partial^2 V_d}{\partial \rho_r \partial T} < 0.$

In fact, $\frac{\partial^2 V_d}{\partial \rho_d \partial T} = \frac{-KB_dB_r}{T^2(MB_d+B_r)} = \frac{\partial^2 V_d}{\partial \rho_r \partial T}.$

Thus as defections increase, the relationship between turnout and the vote grows weaker, vanishing altogether when $(1-\rho_d-\rho_r) = 0$.

Rule 4 asserts that $\frac{\partial V_d}{\partial T}$ decreases when the partisan alignment among eligible voters becomes skewed. To make this proposition more precise, we must investigate the quantity $\frac{\partial^2 V_d}{\partial B_d \partial T}$. Holding T fixed as we let B_d vary yields

$$\frac{\partial^2 V_d}{\partial B_d \partial T} = \frac{(1-\rho_d-\rho_r)K[(B_r-B_d) - (M-1)B_d{}^2]}{T^2(MB_d+B_r)^2} .$$

The partisan alignment associated with the maximum value of $\frac{\partial V_d}{\partial T}$, call it B_{dmax}, is found by setting $\frac{\partial^2 V_d}{\partial B_d \partial T} = 0$. In fact, B_{dmax} varies with the peripherality ratio, M. As M approaches 1, B_{dmax} approaches ½. As M approaches ∞, B_{dmax} approaches 0. The relationship between turnout and the vote thus tends to be strongest in slightly Republican districts, diminishing in strength as the partisan alignment becomes more skewed in either direction. As the peripherality ratio increases, the relationship is strongest in progressively more Republican districts.

Appendix 3. A Note About the Congressional Voting Data

The congressional voting data were collected several years ago by Stanley Kelley, Jr. When Kelley generously offered to share this unique stockpile of information with me, a serious empirical analysis of the relationship between turnout and the vote became possible. The Kelley data are unusual because they include information about the partisan division of registered voters in many congressional districts. Some states do not require all registrants to declare their party affiliations and of those that do, some make it extremely difficult to obtain figures on registration in House districts. In the end, Kelley was able to compile data about registration, voting, and turnout in seven states, though not for all races in those states. His sample includes races from urban, suburban, and rural districts in all regions of the country except the Midwest. It is not, of course, a random sample of all congressional races that occurred in each election year. A list of the congressional districts in Kelley's sample, and the years for which information is available about them, will be provided upon request.

Reassessing Parental Socialization: Indicator Unreliability Versus Generational Transfer

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This article challenges recent socialization research which concludes that the family's influence is primarily limited to the generational transfer of partisan values. We first present a new conceptual approach for studying parent-child agreement. Then, we apply a multiple indicator methodology (LISREL) to operationalize this approach. Reanalysis of the Jennings and Niemi socialization survey finds quite substantial levels of family agreement in areas besides partisanship. These results argue for greater attention to the significance of family socialization, and to the persistence of political beliefs across generations.

Determining how political values are learned and revised through the life cycle is one of the major concerns of political behavior. Yet there is still much uncertainty about the role of various socialization agents and the content of the socialization process. In this situation of flux, one constant concern of research has been the question of the family's role in the learning process. The very nature of childhood focuses attention on the early family environment. During their formative years children have few, if any, sources of learning comparable to their parents (Dawson et al., 1977; Beck, 1977). Moreover, as Davies suggests (1965), the child's almost complete dependence on the parents for basic needs provides little opportunity for resistance to parental pressures to conform. Similarly, parents and children normally share the same cultural, social, and class milieu, providing additional sources of indirect parental socialization. This enormous formative influence suggests that the family has a pervasive effect on an individual's post-childhood thoughts and actions.

The most direct assessment of the family's socialization influence comes from a study conducted by M. Kent Jennings and Richard Niemi (1968, 1974). By separately interviewing a national sample of high school seniors and their parents, these researchers were able to assess directly parent-child agreement and determine the extent of intergenerational value transfer. The Jennings and Niemi study found close family agreement on basic partisan loyalties. However, when they considered relatively specific attitudes, orientations toward politics, political participation, or evaluations of social groups—the degree of congruence between parent and child was minimal. The Jennings and Niemi findings have subsequently been supported by a series of other parent-child pair comparisons (Tedin, 1974; Jennings et al., 1979; Niemi et al., 1978). This research will suggest that this view now needs revision, based upon a merger of new theoretical and methodological approaches to socialization research. The theoretical advances help to clarify the nature and the content of the family socialization process. The methodological advances represent a solution to previously identified weaknesses in socialization research. This new perspective should yield a clearer view of the actual transfer of political values from parent to child.

A New Approach

Because of the frailties and fallibilities of empirical research, one can never directly or perfectly measure the values of parents or children. Instead, empirical research measures quantities which serve as observable indicators of the underlying concepts of interest. For example, we might be concerned with parent-child agreement on general racial attitudes, but our analyses are based on observable indicators of specific opinions such as attitudes towards blacks, school integration, and housing integration. This distinction between concepts and indicators is well known to behavioral research (Selltiz et al., 1966, Ch. 5; Kerlinger, 1966, Ch. 3), and it bears special relevance to socialization research.

I would like to thank Paul Beck, Kai Hildebrandt, M. Kent Jennings, Greg Markus, Marjorie Mowlam, and Jim Stimson for their comments on earlier versions of this article.

One of the pressing problems of survey research is the imprecise measurement (or unreliability) of our indicators (Asher, 1974a, 1974b; Aachen, 1975; Pierce and Rose, 1974). While reliability is a problem confronting all survey research, it is especially relevant for socialization research (Weisberg and Joslyn, 1977). First, the opinions of the young are often poorly formed and articulated. Therefore, survey responses from children and adolescents are considerably more variable than adult survey responses (Vaillancourt, 1973). Second, other applications of survey research tend to focus on the *relative* influence of predictors and the patterns of relationships, while showing less concern for the *absolute* amount of explained variance because of the acknowledged unreliability of survey data. For socialization research the absolute amount of explained variance is of primary interest. Thus, considerable indicator unreliability would substantially weaken parent-child correlations and *give the appearance of weak generational transfer.*

Focusing on parent-child agreement on political concepts (i.e., broad dimensions of attitudes and values) is also appealing on theoretical grounds. Research on family socialization should be primarily concerned with the learning of basic political values: they may be more easily transferred between generations, and such values may have enduring importance through the child's life span (Weissberg, 1974; Inglehart, 1977). By their very nature, specific attitudes and contemporary issues are more likely to be affected by short-term influences than by family socialization. Moreover, specific issue beliefs are less likely to have enduring importance over time as the political environment redefines issue beliefs. In short, even if specific issue beliefs change, general value dimensions may be relatively constant.

Clearly, then, separating the unreliability of measurement from actual parent-child agreement on basic values should result in a more accurate assessment of the strength of intergenerational transfer. A methodological technique combining elements of factor analysis and causal modeling makes it possible to accomplish this statistical feat.

This methodology is generally known as the "multiple indicator" or "unmeasured variables" method (Blalock, 1971, Chs. 16–23; Hauser and Goldberger, 1971). The basic logic of this approach is that by having multiple indicators of each broad concept, two objectives can be reached. First, by analyzing the correlations *among* indicators of a concept, the reliability of each indicator can be estimated. Second, the correlations *between* indicators of different concepts can provide estimates of the relationship between the concepts themselves.[1]

There are several specific statistical procedures which apply this method. The analyses in this piece are based on the LISREL model (LInear Structural RELationships), a procedure developed by Karl Jöreskog and his colleagues (Jöreskog, 1973; Jöreskog and van Thillo, 1972). The parameters of the measurement model are estimated with a series of structural equations which simultaneously compute all of the parameters of the model in a single process.

Applying the Model

The data for this study are drawn from the 1965 Jennings and Niemi survey of high school seniors and their parents (Jennings and Niemi, 1974).[2] The findings and conclusions of this landmark survey have formed the basis of much of our present knowledge of family socialization. Therefore, a reanalysis of these data may throw new light on a cornerstone of socialization research, as well as highlight the insights which this new methodological approach can add to our understanding of the socialization process.

Partisanship. Socialization research has stressed partisanship as the strongest link in the transmission of political values from parent to child. Because of its early acquisition and its persistence as a political reference point, party attachment is an important political identity for many Americans. Few political orientations have been investigated as extensively in the United States as party identification. Early socialization research suggested a substantial parent-offspring correspondence in partisanship (Hyman, 1959; McCloskey and Dahlgren, 1959; Hess and Torney, 1967). The Jennings and Niemi study also uncovered a high degree of partisan consistency between parents and children (1968, 1974, Ch. 2). About two-thirds of the students share their parents' partisan identity in 1965, and less than 10 percent of the students actually switch loyalties to the party in opposition to their parents'. In correlational terms, there is substantial agreement within families in partisan leanings. Party identification is correlated at .59 (Pearson *r*) between parent and child, and Republican/Democrat vote preferences yield a .58 correlation.[3] These correlations far exceed the other generational comparisons in the study, leading Jennings and Niemi to conclude that party identification is a singular example of pervasive parent-to-child value transmission (1968, p. 183).

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[1] While the multiple indicator methodology is mathematically complex, its logic is relatively straightforward. The estimation of indicator reliabilities is analogous to factor analysis' estimation of indicator correlations with the underlying factor. The estimation of the concept-to-concept link follows the logic of path analysis, where the correlation between indicators equals the product of the paths connecting them. In actuality, the indicator reliabilities and concept links are estimated by a single set of simultaneous equations, rather than as two distinct operations.

Because political science applications are limited in number, the interested reader might wish to consult two other research examples: McPherson et al., 1977; Sullivan et al., 1979.

[2] The data used in this article were made available by the Inter-University Consortium for Political and Social Research. The data for the 1965 Student-Parent Socialization Study were originally collected by M. Kent Jennings and Richard Niemi. Neither the original collectors of the data nor the Consortium bear any responsibility for the analyses or interpretations presented here.

This emphasis on partisanship as a central variable in the socialization process leads to reanalyzing these data with the multiple indicator model. The partisan values of the parent and child are each measured by two indicators: party identification and vote preference. It may seem unusual to treat voting preference, and especially party identification, as imperfect indicators of basic partisan values. Party identification is normally considered synonymous with these values, since party identification is by definition a long-term enduring attachment to a political party. Yet research has shown that even party identification is a partially fallible measure (Converse, 1975, p. 131). Thus the use of two indicators allows us to estimate reliabilities, and to achieve more accurate assessment of partisan values and the partisan agreement between generations than could be achieved with only a single indicator.

Figure 1 presents the results of applying the multiple indicator model to the socialization of partisanship. The circles represent the underlying concept of partisan values, and the concept-to-indicator paths represent the reliabilities of our indicators from the survey.[4] The figure shows that our two indicators serve as fairly reliable measures of both parental and child partisan values, with correlations in the .70 range. Normally, we would expect party identification to be the better indicator of basic partisan values. While this holds true for the student sample, for the parents, voting preference emerges as the strongest indicator. This apparent anomaly reflects a feature of the socialization process already noted by Denney and cited by Converse (1975, pp. 143–44). Children are more likely to cue on the parents' visible partisan behavior of vote choice than on the parents' internal and psychological attachment to a political party. Since the socialization process as a whole is being modeled (not parental values taken alone), LISREL accurately reflects this imbalance in the source of child learning.

More importantly, the figure shows that if multiple indicators are used to remove unreliability of measurement from the estimation of parent-child transfer, then intergenerational agreement on the concept of partisan values (.90) is substantially greater than the Jennings and Niemi findings.[5] In short, a large proportion of the apparent generational differences observed in a simple cross-tabulation of parent and child partisanship can be traced to unreliability of measurement. For example, cross-tabulations indicate that roughly 10 percent of parent-child pairs express differing identifications. This figure is grossly inflated because either parental or child values are often inaccurately measured, producing apparent differences where none exist. Subsequent data from the Jennings and Niemi panel have shown that parents do not have a deterministic impact on child values; further change through life is a natural occurrence (Jennings and Niemi, 1975). However, the data imply that the family's initial impact is far greater than previously assumed.

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[3] Jennings and Niemi predominately relied on Tau-*b* as a measure of association. We will present Pearson's *r* correlations recalculated from the same relationships, since the Pearson *r* is comparable to the coefficients estimated by LISREL.

[4] In actuality, these values are the epistemic correlations which, if squared, yield the reliability coefficients. Epistemic correlations are used because they are analogous to the Pearson correlations between indicators and the concept-to-concept correlations from LISREL.

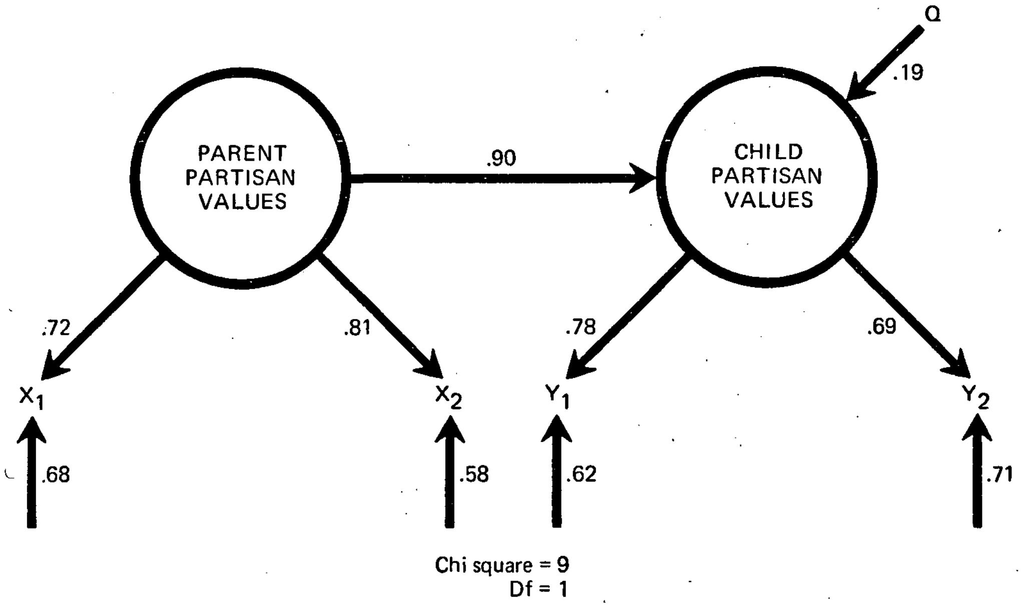
[5] An alternative approach would be to adjust parent-child correlations using reliability coefficients obtained from an independent data source. For example, using panel data, Converse and Markus (1979) estimate the reliability of party identification as roughly .85. We might further assume that child partisanship has a reliability at least .10 lower (Vaillancourt, 1973). Using these figures, the Jennings and Niemi correlations for party identification would still increase from .59 to approximately .75.

While LISREL produces a slightly higher level of family agreement, we believe this is because voting intention of the parent—not party identification—is the more salient partisan cue for the child. Still, even these analyses argue for greater generational transfer than heretofore acknowledged.

Replication of this alternative approach for political efficacy (see Asher, 1974b; McPherson et al., 1977) found essentially identical results to the LISREL model.

Most of the other paths in Figure 1 represent the error terms in the LISREL model. The set of causal paths leading to the parent's indicators and the child's indicators represent factors other than partisan values which determine these specific attitudes, i.e., the error terms of the indicators. The path (.19) from variable Q to the child's partisan values signifies causal influences other than parental values, i.e., the error term of the concept. The overall precision or fit of the model is represented by the chi-square value of 9. Most analysts would interpret this chi-square value as signifying that the hypothesized measurement model is very consistent with the actual data patterns.[6]

In sum, the modest parent-child correlations between observed indicators of partisanship can be primarily traced to the unreliability of measurement (rather than weak generational transfer), which decreases the estimates of actual parent-child agreement by nearly half.[7]



Source: Compiled from data in the 1965 Student-Parent Socialization Study, Inter-University Consortium for Political and Social Research.

Variables in Model:

X_1	Parent Party Identification
X_2	Parent Vote Preference
Y_1	Child Party Identification
Y_2	Child Vote Preference

Figure 1. Intergenerational Transfer of Partisan Values (Standardized Solution)

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[6] With large samples it is nearly always the case that LISREL yields a statistically significant chi-square value. Therefore, analysts have tended to use the chi-square statistic to compare the fit of alternative models. When chi-square is used in an absolute sense, a 10:1 ratio of chi-square to the degrees of freedom is often considered a "good" fit. In fact, most of our models display an extremely good fit, with ratios close to one.

[7] The Jennings and Niemi correlations are nearly half the LISREL correlation. But if we compare explained variance, the imbalance is substantially increased (.35 versus .81). This point is especially relevant to the comparison in Table 1 below.

But so far, the LISREL methodology has merely strengthened Jennings' and Niemi's conclusion that partisan values are transferred between generations. Their other major conclusion is that most other political values are *not* effectively transmitted from parent to child. This conclusion will now be questioned.

Racial Attitudes. Parents obviously play an important role in the formation of racial attitudes. Research has shown that racial images evolve early in life, while the parent is the primary socialization agent. Clearly, the parent provides a rich source of learning cues for racial attitudes. The group references of racial beliefs stimulate parental expressions of racial views, and can also serve to continually reinforce the early formed views of the offspring. Moreover, other evidence shows that racial attitudes are a central element of a belief system, and therefore resistant to later-life changes (Converse, 1964; Stimson and Carmines, forthcoming). In short, given the pervasive presence of racial cleavages in American society and politics, it seems reasonable to expect fairly high generational agreement on these values.

The saliency of racial attitudes was probably at a high point at the time of the 1965 Jennings and Niemi study, coming after several years of intense civil rights activity. On the one hand, this might have acted to strengthen parent-child agreement as the intensity of the issue increased parental cue-giving. On the other hand, popular accounts of the time stressed the emerging generation gap over racial issues. The civil rights movement and the methods of civil disobedience seemed to polarize the generations, which would consequently weaken family agreement.

In fact, Jennings and Niemi found only modest agreement between student and parent views on several indicators of racial values. Opinions on school integration were correlated at .37, and thermometer ratings of blacks yielded only a .26 correlation. More recent data also display only limited intergenerational agreement (Tedin, 1974; Niemi et al., 1978). These findings prompted substantial rethinking among researchers, and some disappointment among those stressing the family as a socialization agent. The accepted conclusion was that excepting party identification, parental impact on other political values was very limited. For policy attitudes such as racial values it was presumed that life cycle effects or other socialization agents exceed parental influences.

However, these same data lead to much different conclusions when analyzed within the multiple indicator framework. For both parent and child there are three indicators of racial values: opinion on school integration, thermometer ratings of blacks, and concern about race as a national problem.[8] The intercorrelations among these indicators were analyzed with the LISREL model, Figure 2. The first finding to emerge is that these three diverse measures are only modestly reliable indicators of underlying racial values, especially for the students. This implies that direct indicator-to-indicator correlations are subject to considerable attenuation. Taking this unreliability into account, there is a very strong generational transfer of racial attitudes (.83)—far in excess of the uncorrected correlations Jennings and Niemi analyzed.

Thus, the significance of separating indicator reliability and actual generational transfer emerges even more clearly for racial attitudes than for partisan values. The increase from .3 correlations to .83 leads to the conclusion that there is far more family agreement than earlier studies realized. Despite the polarization wrought by the civil rights movement and talk of a generation gap on racial values, the family appeared to be a pervasive influence on the early-life formation of racial values for high school students in the sixties.

Civic Tolerance. Although socialization research has devoted considerable attention to the study of partisan and racial values, perhaps the socialization of system norms has aroused even more fundamental theoretical interest. In fact, an initial stimulus for socialization research was the assumption that socialization performed a vital system-maintenance function, perpetuating political norms and the prevailing political culture (Almond and Powell, 1968). The early Chicago studies brought these views into sharp focus (Hess and Torney, 1967; Easton and Dennis, 1969). Their well-known conclusion was that basic commitments to the symbols and procedures of the political system were imprinted early in life.

Subsequent political events and research have raised questions about the pervasiveness of early-life socialization of system norms. Pressed by an intense issue, the same cohort of supportive and conforming children studied by the

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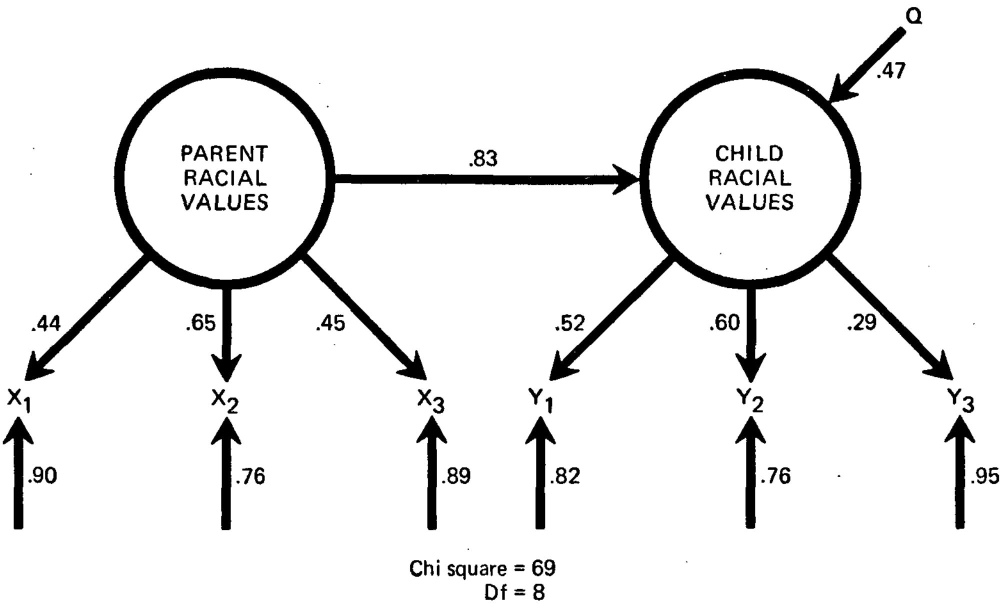
[8] The "race as a national problem" variable is a coding of whether the race problem was mentioned as one of the things the respondent was least proud of as an American. Work by Stimson and Carmines (forthcoming) suggests that racial attitudes might be a multi-dimensional phenomenon, although their analyses do not include all three of our indicators. In this case, the low reliability estimates might also reflect validity problems in the data.



Source: Compiled from data in the 1965 Student-Parent Socialization Study, Inter-University Consortium for Political and Social Research.

Variables in Model:	X_1	Parent Thermometer Rating of Blacks
	X_2	Parent Opinion on School Integration
	X_3	Parent Opinion on Race as National Problem
	Y_1	Child Thermometer Rating of Blacks
	Y_2	Child Opinion on School Integration
	Y_3	Child Opinion on Race as National Problem

Figure 2. Intergenerational Transfer of Racial Values (Standardized Solution)

Chicago group were in the streets as young adults protesting the Vietnam War and challenging political authorities. System support began to take on a short-term image, apparently influenced more by events than by childhood socialization (Miller, 1974).

More specific evidence from the Jennings and Niemi study apparently supports this skeptical view of the socialization of civic norms. This study found only minimal correspondence between generations on two indicators of civic tolerance: allowing an elected communist to take office (r = .13), and allowing anti-religious speeches (r = .07).

Undoubtedly political events do affect political norms. Still, one would hope that support for basic democratic tenets reflects fundamental commitments to these goals which endure through life. This, in turn, would imply the preeminence of early-life socialization and the persistence of supportive norms across generations—but Jennings and Niemi conclude that civic norms are not being learned early from the parent. A possible resolution to this question might again be obtained by separating indicator unreliability from actual generational transfer. Figure 3 portrays the result of the LISREL analyses of parent-child agreement on civic tolerance values.[9] The findings suggest that civic tolerance is a difficult concept to measure, since the model yields the lowest indicator reliabilities of any estimated.[10] By implication, these low reliabilities severely limit the possible magnitude of the correlation between observed indicators. This fact becomes obvious when it is noted that the actual correlation between parent and child civic tolerance values is quite substantial (.44), again, far above the previously assumed "null" relationship.

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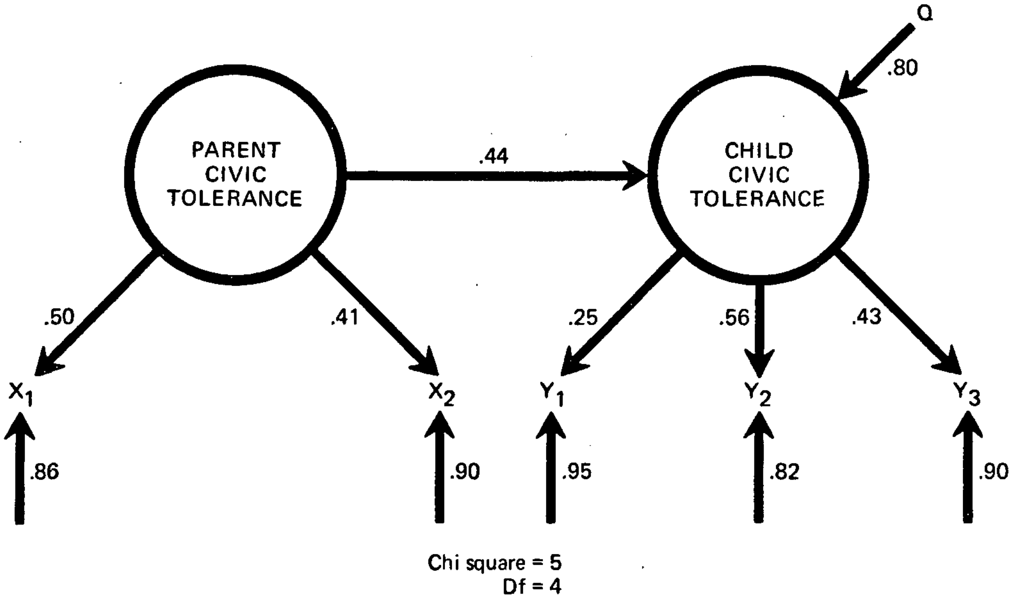
[9] One advantage of the LISREL model is that the set of parent and child indicators need not be identical—if we assume they are equally valid indicators of the underlying concept. Thus, we have added a third indicator of the child's civic tolerance that was not available for the parent sample. When this analysis was repeated using only the two identical questions for both samples, the concept-to-concept correlation was slightly higher (.50).

[10] Recent research by Sullivan and his colleagues (1979) suggests that while there has not been a secular change in the concept of tolerance per se, the objects of tolerance have shifted in time and probably between generations. That is, the parent's tolerance is

Other political norms, such as evaluations of the incumbents of government, may be more closely tied to short-term factors than are attitudes of civic tolerance and other system-level concepts. For civic tolerance, at least, the family appears to play a larger role than previously assumed. But adolescents' attitudes towards the political system are already being influenced by forces outside the family, and these outside forces are likely to increase as children leave their parents' household. Thus the intergenerational continuity of civic tolerance is still lower than for partisan or racial values, implying that these norms represent a blend of moderate intergenerational stability and moderate potential for change.

Trust in People. As illustration of the full range of findings from the multiple indicator approach, one additional detailed example of intergenerational transfer will be provided: trust in people. Although not an explicitly political attitude, personal trust is often viewed as a general orientation that affects political trust (Jennings and Niemi, 1974, pp. 144–45).

Even though little work has been devoted to the socialization of personal trust, we might assume that such basic views of human nature may be formed early in life, and influenced by parental beliefs. Trust in other people forms the groundwork for everyday interactions and is a pervasive, if not highly visible, aspect of behavior. From repeated experience, children may



Source: Compiled from data in the 1965 Student-Parent Socialization Study, Inter-University Consortium for Political and Social Research.

Variables in Model: X_1 Parent Willingness to Allow Anti-Church Speech
X_2 Parent Willingness to Allow Communist to Take Office
Y_1 Child Willingness to Allow Anti-Church Speech
Y_2 Child Willingness to Allow Communist to Take Office
Y_3 Child Belief that All Nations Should Adopt U.S. System of Government

Figure 3. Intergenerational Transfer of Civic Tolerance (Standardized Solution)

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probably best measured by attitudes towards communists and atheists, while the child's tolerance is best measured by attitudes towards the KKK and the Birch Society. If this holds true, we have probably underestimated generational continuity in the concept of tolerance by some margin.

be able to abstract parental beliefs from their behavior.

Jennings and Niemi paid scant attention to these values, even though their study contained three indicators of this concept: trust in people, beliefs in others' being helpful, belief in others' being fair. In replicating their analysis methods, we uncovered only weak parent-child indicator correlations (.05–.10)–implying nearly nonexistent generational agreement.

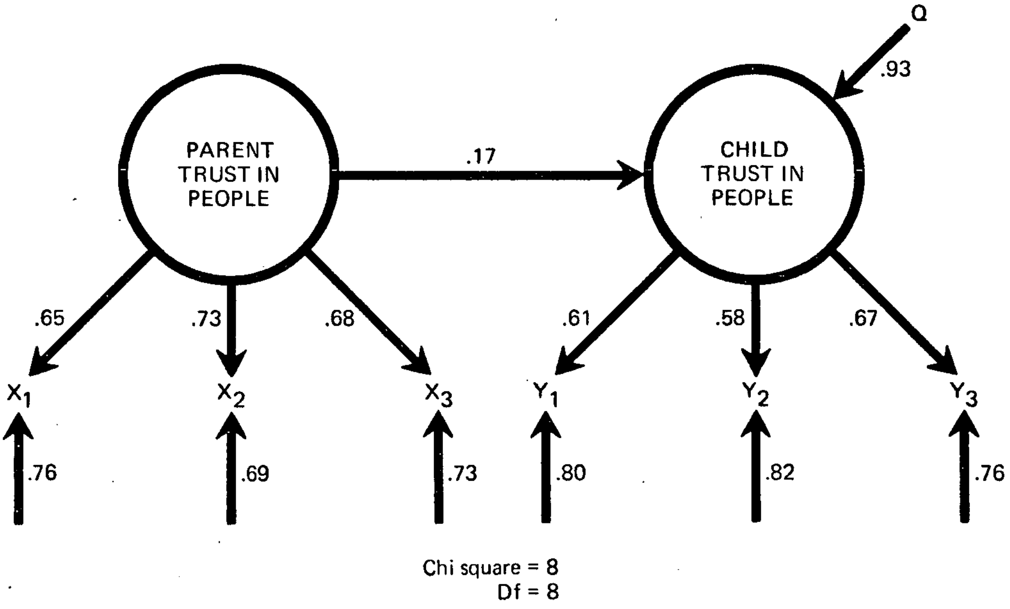
The LISREL model was also applied to these data, but this time without a substantive impact on the analytic conclusions, Figure 4. In the other models the weak parent-child indicator correlations could largely be attributed to indicator unreliability. In this instance the measures of personal trust appear to be highly reliable indicators, second only to the reliabilities of our partisanship model. Therefore, the observed lack of parent-child congruence reflects an *actual* absence of generational transfer–as the .17 concept-to-concept path indicates.

An Overview of Intergenerational Agreement

The preceding analyses have documented a wide range in the extent of intergenerational transfer of political values. This section adds to this evidence by briefly outlining the results of the full inventory of political values measured in the Jennings and Niemi study.

The student-parent study was culled for all possible political values for which the multiple indicator approach was possible. Given the exploratory nature of the study, there are only a limited number of values measured by two or more independent indicators. The LISREL model was applied to each area, and the essential findings are reported in Table 1.

Earlier researchers based their conclusions on the impact of parental socialization upon the indicator-to-indicator correlations in the first column of the table. Indeed, most of these correlations are weak even by survey research standards. However, the admittedly low levels



Source: Compiled from data in the 1965 Student-Parent Socialization Study, Inter-University Consortium for Political and Social Research.

Variables in Model:
X_1 Parent Trust in People
X_2 Parent Belief in People Being Helpful
X_3 Parent Belief in People Being Fair
Y_1 Child Trust in People
Y_2 Child Belief in People Being Helpful
Y_3 Child Belief in People Being Fair

Figure 4. Intergenerational Transfer of Trust in People (Standardized Solution)

Table 1. Comparative Estimates of Intergenerational Value Transfer

	Indicator Correlations	Concept Correlations
Partisanship	.59*	.90
Racial Values	.26*	.83
Political Knowledge	.25	.50
Political Efficacy	.14	.49
Civic Tolerance	.12	.44
Self-esteem	.14	.37
Political Trust	.16	.30
Personal Trust	.12	.17

Source: Compiled from data in the 1965 Student-Parent Socialization Study, Inter-University Consortium for Political and Social Research.

*These values are based on the average of the separate indicator-to-indicator correlations. Other indicator correlations are based on the Jennings and Niemi indices described in *The Political Character of Adolescence,* Princeton, 1974.

of parent-child indicator correlations can be attributed to either of two factors: indicator unreliability or actually weak generational transfer. When unreliability is factored into the estimation of parent-child transfer, there are several instances of considerable generational agreement: partisanship, racial values, political efficacy, and civic tolerance. Each of these areas shows a concept-to-concept correlation in the second column of Table 1 that is far greater than the indicator correlations. To the extent that high correlations are accepted as evidence of causality, parents exert a much larger impact in these areas than recent research has generally concluded. Such strong relationships occur, we believe, because these values involve salient political attitudes where parental cue-giving is more likely, and where early formed attitudes can persist through the life cycle. Moreover, these are also areas where socialization theory had initially stressed the role of the family as maintaining the political values of a system—that is, fundamental political norms, partisanship, and basic policy orientations (Almond and Powell, 1968). The data still do not imply that parents have a deterministic impact on the values of their offspring. But substantial generational continuity in these areas argues that parental socialization at least partially fulfills a system maintenance function.

On the other hand, also uncovered are areas where unreliability has only a minor effect on estimates of value transfer: self-esteem, political trust, and personal trust. In some instances these values are characterized by high levels of indicator reliability so that unreliability adjustments do not substantially rise above the indicator-to-indicator correlations. More fundamentally, some political values—such as trust in the incumbents—should be more dependent on short-term influences outside the family. New issues, or low salience political topics should also be expected to show weak generational transfer (even with the multiple indicator approach), because by their very nature these are not areas where the family can exert a strong and enduring influence. Furthermore, we would argue that high parent-child correlations for such values are not sufficient proof of family socialization, but may reflect the spurious effect of environmental factors simultaneously influencing parent and child values. In short, the generational transfer of political values should be weak in these areas, and other agents of political learning should be more important than the family.

Discussion

In general, socialization researchers have viewed the results of the Jennings and Niemi study as playing down the role of the family as a socialization agent. Such pessimism now seems premature. Application of a more rigorous, and appropriate, methodology to these data yields a substantially different perspective on the socialization process. Separating indicator reliability from the estimation of generational transfer, there are quite substantial levels of family agreement in several instances. Indeed, Jennings and Niemi were also aware of this problem in their design:

> As in most socialization research, many of the dimensions we used are comprised of "soft" variables. They are neither as reliable nor as valid as they should be. While we cannot estimate just how much measurement error this "softness" introduces, it is certain that the presence of such error . . . lowers the parent-student correlations from their true values (1974, p. 320).

We have not, therefore, offered a new idea so much as a partial solution to an old problem.

The weight of the evidence rests heavily upon the distinction between "indicators" and "concepts." While it is easy to acknowledge the unreliability of indicators, it is more difficult to identify confidently the nature of the resulting "purified" concept. On the one hand, the slippage in the concept-to-indicator link may primarily indicate that attitudes are only weakly formed, or nonexistent, for sizeable numbers of parents and children. Converse's work on the nature of mass belief systems would support this interpretation (Converse, 1964, 1970, 1975). In this light, the findings would be summarized by saying that *to the extent that individuals have attitudes,* a strong intergenerational transfer of these values exists.

Recent research, however, offers another interpretation of these findings. Aachen argues that the apparent instability of mass attitudes is due more to the imprecision of survey questions than to the absence of attitudes (Aachen, 1975). Similarly, several recent reports show that improvement in the question formats of the Michigan election studies have significantly increased the correlations between various political attitudes (Bishop et al., 1978; Sullivan et al., 1978). This research consequently offers a different interpetation of our findings—*to the extent that individual attitudes can be accurately measured,* a strong generational transfer of these values exists.

This research is thus related to this ongoing dialogue on the nature of mass belief systems. To some extent the conclusions of this research will rest on the ultimate resolution of this controversy. But even with this uncertainty about the interpretation of the findings, there are several specific inferences that can be drawn.

The multiple indicator approach provides a new perspective for examining variations in the strength of the generational transmission process. The correlation between observed parent and child indicators will vary with the reliability of the parent indicator, the reliability of the child indicator, and the actual transfer from parent-to-child. Control variables—such as family authority patterns, political interest, closeness to parent, or the frequency of family political discussions—can affect one or several of these links. For example, family partisan agreement was reanalyzed while controlling for the *child's* level of political interest. Politically interested children tend to have more strongly held attitudes; thus indicator reliability is higher for this group than for the low-interest group. In addition, the high-interest group is also more likely to mirror the partisan values of their parent (.94) than are uninterested children (.82). Moreover, the effects of this control variable are localized on the child's attitudes, with only minor variation in the reliability of the parent's partisan indicators across control groups.

Similarly, Tedin shows that issue salience increases parent-child agreement across a range of indicators (1974). With our new approach we would want to determine whether this is because issue salience increases the reliability of measurement, or because salience increases parental cue-giving and child receptivity on the issue and thus strengthens actual generational transfer. Perceptual accuracy also affects indicator-to-indicator correlations (Tedin, 1974). But it should be determined whether the reliabilities or intergenerational links are primarily affected by perceptual accuracy. In the end, this approach will allow us to remove the confounding effects of measurement error to come to a more precise understanding of what actually affects the process of political learning.

Finally, socialization researchers have interpreted the magnitude of parent-child agreement as a reflection of the extent, or likelihood of, political change in a society. Thus, with their low agreement levels Jennings and Niemi stressed the openness of the American system to change, and the need to focus research on change factors or agents of political change. Indeed, even our findings acknowledge the substantial potential for political change between generations, especially in areas where generational transfer was weakest.

By no means do the results imply a process of generational determinism—that is, children faithfully duplicating their parents' views. However, this reassessment of the actual extent of generational transfer finds that parents exert a strong formative influence on key values of their offspring. These results should lessen emphasis on the theme of social change, and restore attention to the significance of family socialization and to the persistence of political beliefs across generations.

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Implications from the Disequilibrium of Majority Rule for the Study of Institutions

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While contemporary political science (as, for example, in such subjects as political socialization, studies of public opinion, etc.) tends to emphasize the study of values and tastes (because of an assumption that political outcomes–like market outcomes–are determined by the amalgamation of individual preferences), the older tradition of political science emphasized the study of institutions. The line of research in political theory followed during the last generation has involved seeking an equilibrium of tastes; but it has revealed that such an equilibrium exists only rarely, if at all. The inference then is that prudence in research directs the science of politics toward the investigation of empirical regularities in institutions, which, though congealed tastes, are "unstable constants" amenable to scientific investigation.

Social scientists are now, and probably always have been, of divided opinion about the degree to which institutions as well as personal values, opinions, and tastes affect the content of social decisions. (I use the words "values," "opinions," and "tastes" interchangeably, not because they mean exactly the same thing, but because the processes by which they can influence decisions are identical.) It is clear that the values of at least some members of society do ineradicably influence these decisions. Even when it is claimed that God, or the law of nature, or the sovereign people, or the working class, or some other abstract non-human entity determines outcomes, it is still true that some members of society, say, priests, judges, parliamentmen, dictators, etc. must interpret what the abstraction directs. In the most extreme cases, the voice the people hear and the words spoken come immediately from the Delphic priestess, not from the god who is said to inspire her. That being so, we can never leave out the influence of some person's values and tastes on social decisions. Even if the priestess is unintelligible, the priestly interpreter tells the supplicants what to do.

On the other hand, we cannot leave out the force of institutions. The people whose values and tastes are influential live in a world of conventions about both language and values themselves. These conventions are in turn condensed into institutions, which are simply rules about behavior, especially about making decisions. Even the priestess in her frenzy probably behaves according to rules and, for certain, her interpreter is constrained by specifiable conventions. So interpersonal rules, that is, institutions, must affect social outcomes just as much as personal values.

Ambiguity arises, however, when we attempt to assess the relative significance of these two kinds of forces. Very probably, both are necessary and neither is alone a sufficient condition for outcomes. If so, a full statement of social causation must include them both. But, nevertheless, it is often believed to be convenient and practically useful to assume that one force (either the personal or the impersonal) is constant, while the other is variable and thus in some sense marginal and "more significant" than the other. With this assumption, if the institutions are constant, then one can predict outcomes from tastes, or, if tastes are constant, then one can predict outcomes from institutions. It is of course true that this easy predictability is an illusion–but it is an illusion by which many scholars are hoodwinked because in quiet times the institutions are constant and only tastes are in dispute, while in turbulent times the institutions are in flux and only human greed seems constant. One fundamental and unsolved problem of social science is to penetrate the illusion and to learn to take both values and institutions into account. In the last generation we have made some small progress in this direction, if only to acquire a bit of sophistication about the problem, and the purpose of this article is to chronicle this progress.

Methodological Traditions in Political Science

Political science draws almost equally on traditions that overemphasize institutions and traditions that overemphasize tastes, which is perhaps why political scientists seem so eclectic as compared to, say, sociologists (whose tradition is almost exclusively institutional) or to economists (whose recent tradition, at least, stresses tastes).

The emphasis on institutions is our classical heritage. Aristotle collected and described 150 constitutions because he believed that constitutions determined both social outcomes and individual character. Even Plato, who initially argued for the rule of men rather than the rule of law, neverheless devoted most of the *Republic* to a description of the institutions necessary to produce the kind of people he wanted for rulers, thereby implying that the institutions were primary and the particular rulers merely intermediaries between the institutions and the outcome. The notion that the quality of men's character is controlled by the laws under which they live, a notion that comes to us from ancient Greece by way of Roman law and eighteenth-century philosophers like Montesquieu, is, consequently, very much a part of contemporary political science. For example, it is said, with astonishing variety, that both the welfare state and restraints on government make people free, productive, contented, and self-respecting; or, for another example, it is said that incompatible varieties of economic institutions such as capitalism and socialism make people better off both morally and economically. Doubtless, the most extreme and absurd of the modern versions of classical institutionalism is Marxism, a picture of society in which economic institutions determine not only individual character but also the whole course of human history.

The emphasis on taste and values, on the other hand, is our Christian heritage. Because Christianity based the social order on personal decisions about faith and love and because it rejected the Judaic system of rules and forms (which came close to classical institutionalism), Christian theologians—at least from the Middle Ages onward—insisted that the quality of social outcomes depended almost entirely on the moral quality of rulers: Christian kings make good decisions; pagan or irreligious kings do not. Even in this secular century, some writers directly in the Christian tradition have described society in exactly this way. For example, T. S. Eliot, responding to the Munich crisis with *The Idea of a Christian Society* (1940, p. 34), saw only a change in *beliefs* as a way out of the world crisis: A community of Christians in a Christian state governed by rulers who accept Christianity, "not simply as their own faith, . . . but as the system under which they are to govern." It would be hard to find a more complete conviction that social outcomes (and personal character) are determined by what people believe.

Owing, however, to the lack of interest in theology among twentieth-century intellectuals, the contemporary force associating individual values and social outcomes is wholly secular, though probably derived (as is, for example, extreme methodological individualism) from Christian modes of thought. In the ideology of democracy, which may well be a kind of secularized Christian theology, that form of government is often, though I believe quite inaccurately, defined as the rule of the people—by which it is meant that the people's values solely determine public decisions. For reasons that I have discussed at length elsewhere, this picture of democracy is internally inconsistent and cannot be sustained (Riker, 1978, 1980). At most democracy involves a popular veto on rulers, not a popular rule. Nevertheless inconsistencies and inaccuracies do not deter most ideologues, so that it is probably the case that nowadays the most widely accepted interpretation of democracy is that it is a device to combine individual values into decisions of government. Furthermore, this understanding, which went by the name of Benthamite "radicalism" in nineteenth-century England and of "popular sovereignty" in nineteenth-century America, is today believed by huge numbers of people (incorrectly, of course) to describe what actually happens in democratic governments.

While this supposed political description is mere ideology, it is nevertheless an important part of some contemporary political science and contributes greatly to the scientific emphasis on tastes and values, an emphasis expressed, for example, in the great amount of research on public opinion (which concerns the nature of tastes and values), political socialization (which concerns the creation of tastes and values), and representation (which concerns the incorporation of tastes and values in public decisions).

Great as is the contribution of democratic ideology to an emphasis on tastes and values, there is an even greater contribution, I believe, from the example of microeconomics. The theory of price in a competitive market—one of the few well-tested and verified theories in all of social science—is a theory in which institutions (i.e., the market) are held constant, while tastes determine outcomes. The theory takes this form: Given an auction market for a continuously divisible commodity with several buyers and several sellers whose tastes are constant over the period of the auction, then the price of the commodity is jointly and completely determined in a particular, describable way by the sum of the buyers' desires to buy and the sum of the sellers' desires to sell.

(These desires are, of course, tastes and values.) Furthermore, as long as tastes are constant the price so determined is a Pareto-optimal equilibrium in the sense that no *pair* of traders would agree to depart from it because any departure in favor of one trader would hurt another.

Since this theory admits prediction of an equilibrium and since the actual occurrences of numerous predicted equilibria have been verified, the prestige of this theory is higher, I believe, than that of any other theory in the social sciences. Indeed, it seems to me that this success alone (i.e., predictions from the theory of price) elevates the science of economics above all other social sciences in popular esteem among intellectuals, and renders economists believable even when they write of totally different subjects such as macroeconomics and social welfare, about both of which subjects their theories are as unverified as most others in social science. The scientific and intellectual success of price theorists in discovering equilibria has, of course, led many other social scientists to emulate them. We see, therefore, searches for equilibria of tastes in all branches of social science, not least of all political science.

The Search for General Equilibria

As I noted at the beginning of the previous section, political science draws eclectically both on traditions that overemphasize institutions and on traditions that overemphasize tastes. But, in the last 30 years or so, it seems to me that the traditions overemphasizing tastes have predominated. The study of constitutions, which characterized political science in the first half of the century, has latterly given way to the study of political culture, political behavior, and public opinion, all of which concern values and tastes. Simultaneously, with a kind of unspoken intellectual coordination, political theorists have analyzed the conditions for equilibria in abstract majority voting systems, which are in fact the conditions for an equilibrium of values. This development in political theory, which is described in this and subsequent sections, has by now revealed precisely what kind of equilibria can be expected, thereby allowing us to understand, with much more sophistication than was previously possible, the relation of values and institutions in structuring political outcomes.

The beginning of the search for conditions of equilibria is Duncan Black's rediscovery (in the mid-1940s) of the paradox of voting. Before that time scholars had indeed often discussed the equity and effect of voting systems, especially in disputes over proportional representation, methods of nomination, and forms of ballots. But, so far as I have been able to discover from a desultory survey, hardly anyone had recognized that the supposed defects might be based on individual tastes rather than structures of systems. If based on tastes, the defects are irremediable because, given an appropriate distribution of tastes, even a perfected system of voting might produce imperfect results. This is precisely the inference one draws from an analysis of the paradox and it is perhaps owing to an unspoken, even unrecognized, repugnance at this deduction that, when the paradox was initially discovered by Condorcet and rediscovered by Lewis Carroll, E. J. Nanson, and E. V. Huntington, it was nevertheless ignored (or perhaps repressed) by political scientists (see Black, 1958, pp. 156–238; Riker, 1961). Once Black brought the paradox irrepressibly to scholarly attention and showed that the disequilibria inherent in it depended not on the institutions of voting but on the distributions of taste, the search for conditions of equilibrium seemed an intellectual necessity.

To discuss this question, one needs an abstract society of these elements:

1. Alternatives: $\{a_1, a_2, \ldots, a_n\}$. If $n = 2$, equilibrium is certain because a_1 beats or ties a_2 or vice versa. Problems of equilibrium arise, however, when $n \geqslant 3$.

2. Voters: $\{1, 2, \ldots, m\}$. When $m = 1$, the problem is trivial, so it should be that $m \geqslant 2$.

3. Preference: Assuming that voters can compare alternatives and value some of them more than others, there are binary relations of preference, P_i (where $i = 1, 2, \ldots, m$), of indifference, I_i, and of the two combined, R_i, expressing a voter's estimate of the relative value of any pair of alternatives. (One writes "$a_j\ \mathrm{R}_i\ a_k$" to mean "voter i prefers a_j to a_k or is indifferent between them.") Conventionally, R is assumed to be reflexive ($a_j\ \mathrm{R}_i\ a_j$), connected (either $a_j\ \mathrm{R}_i\ a_k$ or $a_k\ \mathrm{R}_i a_j$), and transitive (if $a_h\ \mathrm{R}_i a_j$ and $a_j\ \mathrm{R}_i\ a_k$, then $a_h\ \mathrm{R}_i\ a_k$), so that by R (or P or I) a voter orders any triplet of alternatives from best to worst: $a_h\ \mathrm{R}_i\ a_j\ \mathrm{R}_i\ a_k$ or $a_h\ a_j\ a_k$. [Notationally, $a_h\ \mathrm{P}_i\ a_j\ \mathrm{I}_i\ a_k$ is written: $a_h\ (a_j\ a_k)$.] Perhaps the attribution of the ability to order places unwarranted confidence in the fragile human ability to concentrate. By the assumption of the transitivity

of R, however, we give voting on values a chance at equilibrium with the best of human participation. If, then, voting fails, it fails the easiest possible test.

4. Outcomes from voting: Given a society of m voters faced with n alternatives, there are $(n!)^m$ possible profiles of preference D, that is possible ways the members of the group can individually order the alternatives. (That is, there are $n!$ possible orders of n alternatives and each of the m voters can select one of those orders.) The operation on a profile, D, of majority voting on pairs of alternatives yields an outcome relation, M, where "a_h M a_k" means: "(the number of i such that a_h P_i a_k) $\geqslant$ (the number of i such that a_k P_i a_h)," assuming, of course, that no i such that a_h I_i a_k participates in the voting. M may be, but need not be, a transitive relation so that M may yield either some one of the $n!$ orders of alternatives or intransitive cycles like a_h M a_j M a_k M a_h.

To consider the problems involved in the summation of preferences, observe the profiles, D^1 to D^4, where $n = m = 3$:

D^1	D^2
1. $a_h\, a_j\, a_k$	1. $a_h\, a_j\, a_k$
2. $a_h\, a_k\, a_j$	2. $a_j\, a_h\, a_k$
3. $a_j\, a_k\, a_h$	3. $a_k\, a_h\, a_j$
a_h M a_j M a_k	a_h M a_j M a_k

D^3	D^3
1. $a_h\, a_j\, a_k$	1. $a_h\, a_k\, a_j$
2. $a_j\, a_k\, a_h$	2. $a_j\, a_h\, a_k$
3. $a_k\, a_h\, a_j$	3. $a_k\, a_j\, a_h$
a_h M a_j M a_k M a_h	a_h M a_k M a_j M a_h
"forward cycle"	"backward cycle"

In profiles D^1 and D^2 there is a decisive winner by M in the sense that a_h beats each of $n - 1$ other alternatives. But in profiles D^3 and D^4, which are examples of the paradox of voting, there is no decisive winner because in, say, D^3, a_h will win if the sequence of voting is a_j versus a_k and then a_j versus a_h, a_j will win if the sequence is a_k versus a_h and then a_j versus a_k, etc., or no alternative will win if a round robin is conducted. The absence of a decisive winner is particularly disconcerting because, after assuming that each voter can order his or her values, it turns out that the group of voters cannot order them. Indeed, the people are coherent but the group is incoherent.

It is precisely the absence of a decisive winner that constitutes disequilibrium and the paradox of voting shows that disequilibrium can occur with majority voting, the relation M. Furthermore, the possibility of this kind of disequilibrium is present in *any* fair voting method, so it is not the institution of M, but the distribution of tastes, that is at fault (Arrow, 1963). One is consequently driven to ask what properties distinguish profiles with an equilibrium outcome (like D^1 and D^2) from profiles without one, like D^3 and D^4. (This question has usually been posed with respect to the relation M, so I will restrict my discussion to it. But, by reason of Arrow's theorem, we know that similar questions could be raised about any voting method, say, positional methods like plurality voting or approval voting.)

During the late 1960s, a systematic answer was developed, based on the observation that, for the 3! orders of a triplet of alternatives,

1. $a_h\, a_j\, a_k$
2. $a_h\, a_k\, a_j$
3. $a_j\, a_h\, a_k$
4. $a_j\, a_k\, a_h$
5. $a_k\, a_h\, a_j$
6. $a_k\, a_j\, a_h$,

numbers 1, 4, and 5 constitute D^3 and result in the forward cycle and numbers 2, 3, and 6 constitute D^4 and result in the backward cycle. Hence D^3 and D^4 exhaust the ways in which intransitive triples can occur. Any conditions on orderings by individuals such that either $a_h\, a_j\, a_k\, a_h$ or $a_h\, a_k\, a_j\, a_h$ are rendered impossible by M is thus a guarantee of equilibrium. One such condition is, for example, that, for any number of voters, some alternative in a triple is never in first place in a voter's order (as a_k is not in D^1) or some is never in last place (as a_h is not in D^2) or never in the middle place (as a_h is not in D^1)—this is the condition of "value restriction" (Sen, 1966). Or another condition is "extremal restriction," which is that, for any number of voters and for some order $a_j\, a_h\, a_k$ in D, if another order has a_k first, then this other order must have a_j last (as in D^2, $a_j\, a_h\, a_k$ is voter 2's order and $a_k\, a_h\, a_j$ is voter 3's) (Sen and Pattanaik, 1969). An exhaustive list of similar conditions is set forth in Fishburn (1973).

In addition to their completeness, the merit of these conditions on profiles is their clear revelation that equilibrium depends entirely on the accident of a non-cyclical set of voters' preferences. The defect of these conditions is, on the other hand, their failure to indicate the likelihood that tastes might or might not be cyclical. Lacking that indication, they do not admit assessment of the practical significance of disequilibrium. Fortunately there exist less complete but intuitively more vivid geometric or topological conditions for equilibrium that do allow practical interpretation.

Historically, the first such condition, single-peakedness, was devised by Black even before Arrow's theorem was formulated (Black, 1948). If alternatives are arranged on a horizontal axis and the voters' ordinal valuation is measured on a vertical axis, then a voter's ordering may be represented as a curve on the positive quadrant connecting the voter's valuation of alternatives (see Figure 1). Such a preference curve is single-peaked if, as it flows from left to right, it is always rising, always falling, or rising to a peak or a plateau and then falling. By an appropriate arrangement of alternatives on the horizontal axis, any ordering of alternatives may be expressed as a single-peaked curve. If, however, three or more preference curves are drawn above a particular arrangement of alternatives on the horizontal axis, it may happen that all curves cannot be single-peaked. (See Figure 2, in which the reader may verify that, no matter how the three alternatives are ordered on the horizontal axis, at least one of the three curves from D^4—a cyclical profile—must fail to be single-peaked. In the particular ordering of Figure 2, voter 3's curve fails; but, were the ordering on the horizontal axis to be, say, a_h a_k a_j, then voter 2's curve would fail.)

Black's discovery was that, if any ordering on the horizontal axis exists such that all voters' preference curves are single-peaked, then an equilibrium exists in the sense that one alternative can beat or tie $(n - 1)$ others. Moreover the winning alternative(s) can be specified: Identifying the alternative beneath the peak of voter i's curve as O_i (for "optimum for i") and numbering the optima so that O_1 is at the far left and O_m at the far right, then, if m is odd, $O_{(m+1)/2}$ wins and, if m is even, $O_{m/2}$ and $O_{(m+1)/2}$ tie. For proof, when m is odd, note that, if a curve is rising from a_h to a_j, then the voter prefers a_j to a_h and, conversely, when the curve is falling, the voter prefers a_h to

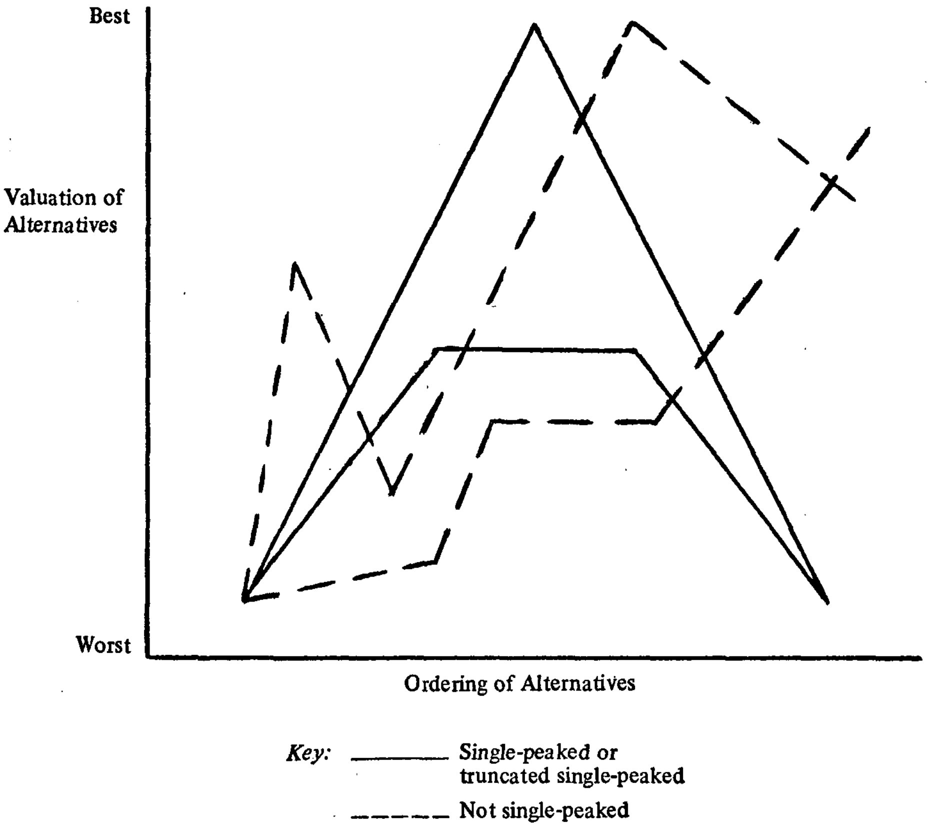
Best
Valuation of Alternatives
Worst
Ordering of Alternatives
Key:
Single-peaked or truncated single-peaked
Not single-peaked

Figure 1. Preference Curves

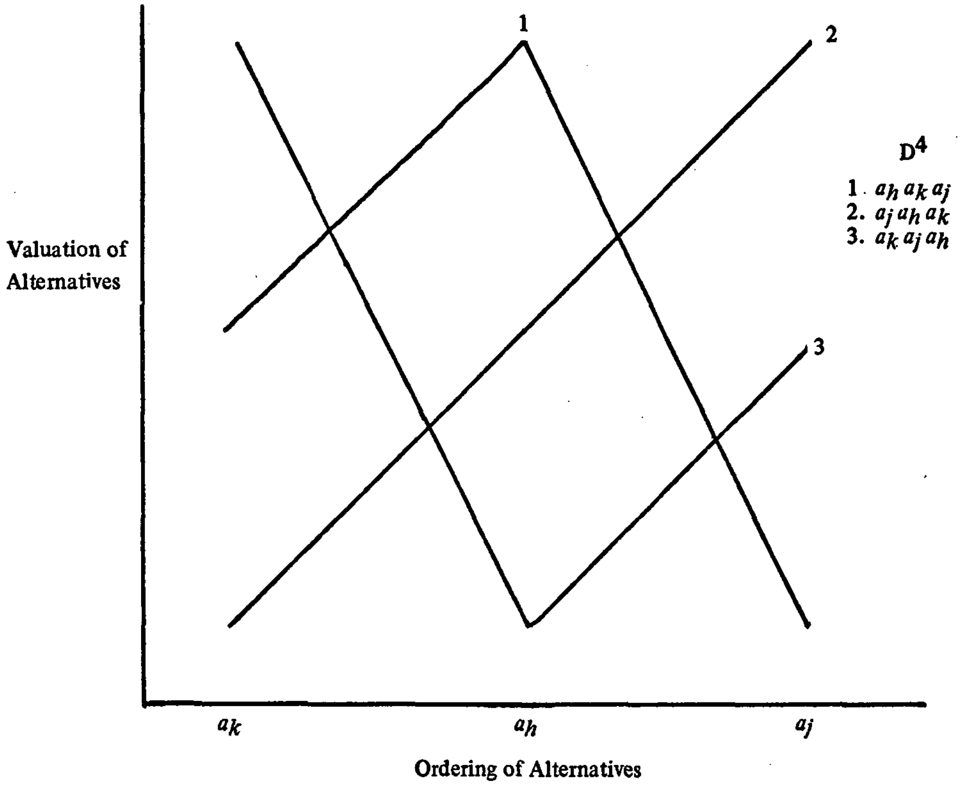

Figure 2. Non Single-Peakedness

a_j. Placing some a_k to the left of $O_{(m+1)/2}$ against $O_{(m+1)/2}$ in a vote, note that $O_{(m+1)/2}$ wins because over half the curves are rising between a_k and $O_{(m+1)/2}$, specifically all those curves with optima numbered $O_{(m+1)/2}$ to O_m (which is a majority itself) and all curves with optima lying between a_k and $O_{(m+1)/2}$. Similarly, placing some $a_{k'}$ to the right of $O_{(m+1)/2}$ against it, note that $O_{(m+1)/2}$ wins because over half the curves are falling between $O_{(m+1)/2}$ and $d_{k'}$, specifically all those from O_1 to $O_{(m+1)/2}$ (a bare majority) and those with optima between $O_{(m+1)/2}$ and $a_{k'}$. A similar argument establishes that $O_{m/s}$ and $O_{(m+1)/2}$ tie when n is even.

It should be noted that this equilibrium at the median optimum is characterized by a balancing of opposites, a feature found in all other geometrically defined equilibria of voting. There are an equal number of voters on either side of the median, which is why it is the equilibrium. Suppose one subtracts (or adds) two voters whose optima are on opposite sides of the median, then the equality is unaffected and the equilibrium is characterized in some fundamental way by a pairing of opposites.

As a condition of equilibrium, single-peakedness (like the previously mentioned conditions, all of which were, however, discovered later) guarantees that neither a_h M a_j M a_k M a_h nor a_h M a_k M a_j M a_h occurs. It has the additional merit, moreover, of revealing a rationale for the existence of equilibria: That all curves are single-peaked means that all voters judge the alternatives consistently with respect to one issue, namely, that measured by the dimension on the horizontal axis. They may, of course, disagree about the best position on the issue, but they do agree that this single issue is the relevant basis for judgment.

This is why this condition has an intuitively obvious application to political campaigns, as in Downs' proposition that party platforms in a two-party system converge to the values of the median voter (1957, pp. 114–25). While Downs derived this argument from an economic model of the spatial location of firms, still his argument for equilibrium at the median voter's optimum assumes single-peakedness and is indeed invalid without it. This application suggests just how restrictive the condition is in practice because it seldom appears to be satis-

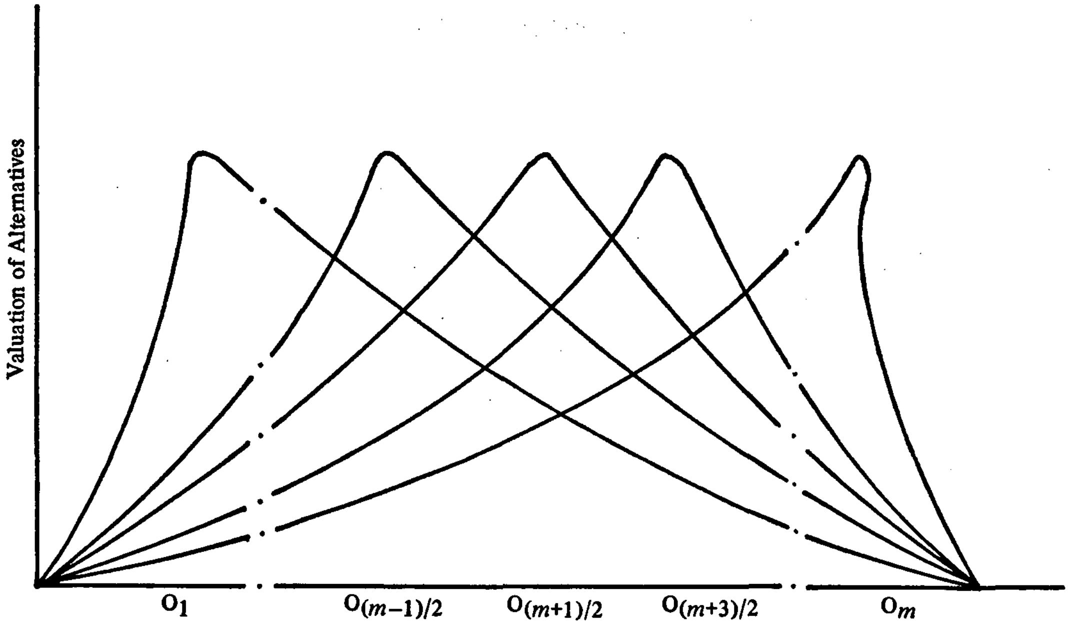

Figure 3. Voter's Optima

fied in the real world (Robertson, 1976). Despite the frequent journalistic use of dichotomies, e.g., "left-right," "Catholic-Protestant," "Fleming-Walloon," etc.–all of which are extremes on one issue dimension–still, scholarly efforts to describe real politics on one dimension seem always to break down. Indeed, once Downs set forth his model, it seemed so inadequate that other theorists soon developed an *n*-dimensional analogue (Davis and Hinich, 1966; Davis, Hinich, and Ordeshook, 1970).

Given the intuition that the one-dimensional model is inadequate for description, the appropriate next step is to search for equilibria in two-dimensional and ultimately in *n*-dimensional issue spaces. Black and Newing (1951) started the search with three voters in a two-dimensional model in which the peaks of Figures 1, 2, and 3 have become the humps of Figure 4. The vertical axis in that figure is, like the vertical axes in previous figures, for the ordinal measure of valuations. The two horizontal axes are issue dimensions or bases of judgment that define a plane (rather than a line) on which alternatives are located. Voter *i*'s optimum (O_i) lies in that plane directly beneath the highest point of the hump. Curves *e* and *f*, which are reflections into the x_1 x_2–plane of two levels of preference, are indifference curves in the sense that voter *i* prefers all alternatives in the open space between *e* and *f* to any alternative on *e* but *i* is indifferent among all alternatives on, say, *e*.

To search for an equilibrium we need look only at the indifference curves in the x_1 x_2–plane, as in Figure 5. Between two voters, with optima at O_1 and O_2, is a "contract curve" which connects points of tangencies of the voters' indifference curves. All the points on which voters 1 and 2 might agree lie on the contract curve. To see why, consider point a_k which lies, for each voter, on the outer of the two sets of indifference curves displayed. By definition, all points in the open shaded space are preferred by *both* voters to a_k. By successive reduction of the shaded area, one arrives at some point on the contract curve. When there are three voters, however, agreement is less easy to arrive at. Observe in Figure 6 that, while voters 1 and 2 might agree by majority vote on a_k, still a_j M a_k by voters 2 and 3. Nevertheless a_h M a_j (by 1 and 3) and a_k M a_h (by 1 and 2), so a cycle exists and there is no equilibrium.

There is, however, some chance for equilibria in this situation: If O_3 were to lie on the contract curve between O_1 and O_2, say, at a_k, then $O_3 = a_k$ would be a median between O_1 and O_2 and hence preferred by some pair (either 1 and 3 or 2 and 3) to *any* other point in the plane. In general, if one voter's optima lies on a contract curve between two others, then there is an equilibrium outcome. Note

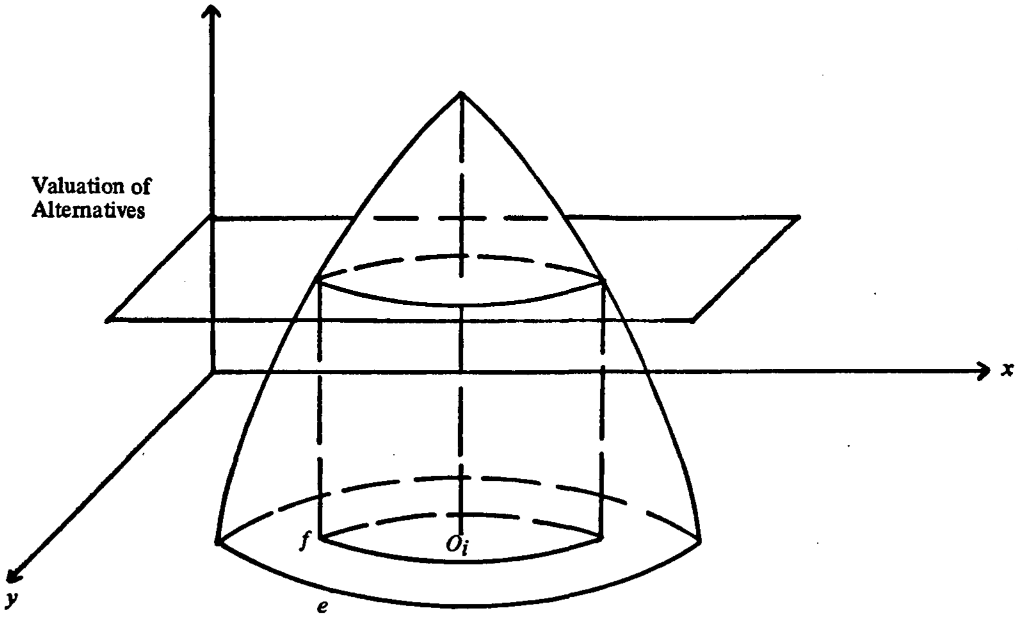
Valuation of Alternatives
x
y
f
O_i
e

Figure 4. Single-Humped Valuation of Alternatives in Two Dimensions (*x*,*y*)

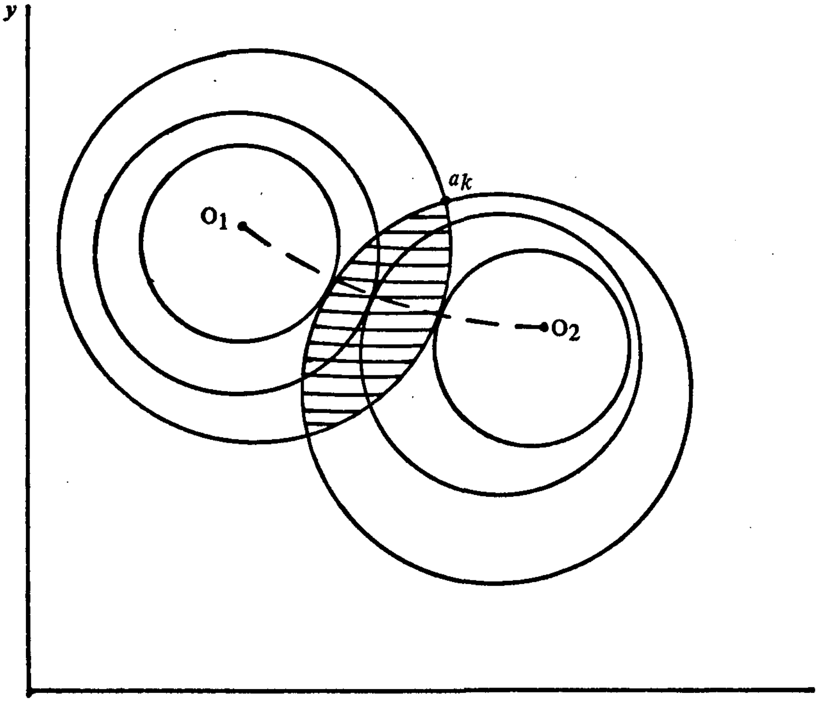

Figure 5. Indifference Curves

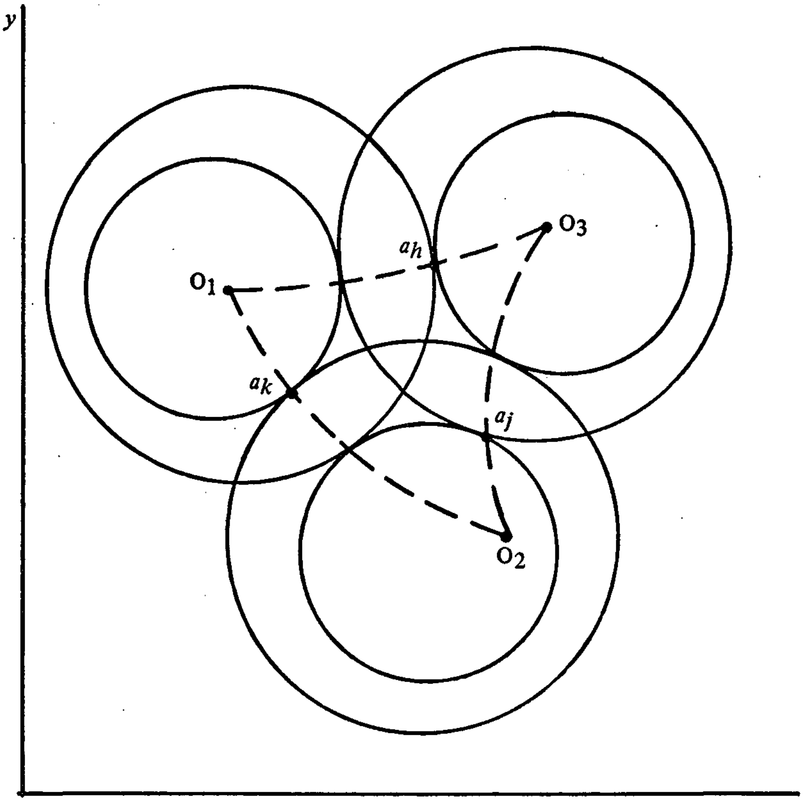

Figure 6. Contract Curves for Three Voters

that, as in the one-dimensional case, there is a balance between opposites, because a_k would remain the equilibrium if O_1 and O_2 were removed or if additional pairs of optima were added, one of each pair on the O_1 O_2 contract curve between a_k and O_1 and the other between a_k and O_2. This discovery was generalized by Charles Plott (1967). For the expanded situation we need some new notation. Since we now assume an infinite number of alternatives and continuously differentiable utility (as a measure on individual preference), we can no longer use n for the number of alternatives. Rather we use it to identify dimensions by which voters judge alternatives, 1, 2, . . . , n-dimensions. Then $U^i = U^i(x_1, x_2, \ldots, x_n)$ is the utility to the i^{th} voter of some point, a, in n-dimensional space. Let there be a status quo alternative, $\overline{a}$, that is, the alternative currently in force and let some a_j, which is a "small" distance, d, from $\overline{a}$, be placed against $\overline{a}$ in a majority vote. If $a_j \text{ P}_i \overline{a}$, voter i obtains an increase in utility from a_j over $\overline{a}$. Let ΔU^i be the measure in utility of a vector in n-space from $\overline{a}$ toward some (unspecified) other point. Specifying the other point as a_j, one can say that, if $\Delta U^i a_j > 0$, then voter i prefers a_j, that, if $\Delta U^i a_j < 0$, voter i prefers $\overline{a}$, and if $\Delta U^i a_j = 0$, voter i is indifferent between a_j and $\overline{a}$. If there is some set of voters of size $\frac{m+1}{2}$ such that the gradient vectors, of utility for each voter, i, in the set are $\Delta U^i a_j \leqslant 0$, then a majority prefers $\overline{a}$ to a_j and $\overline{a}$ is a Condorcet winner or equilibrium.

A set of sufficient conditions for $\overline{a}$ to be in equilibrium are, for m odd:

1. that indifferent voters do not vote on a motion;
2. that there is at least one voter, i, for whom $\overline{a}$ provides the maximum utility;
3. that the $m-1$ remaining voters—an even number—can be divided into pairs, i and i', the interests of voters i and i' are diametrically opposed in direction and amounts of utility.

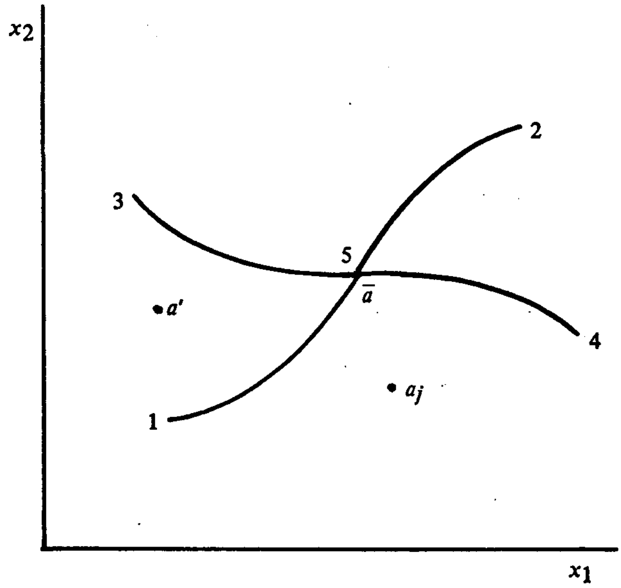

Figure 7. Equilibrium for m=5 Voters in n=2 Space

It is intuitively evident (and proved in Plott [1967]) that for m voters for which conditions 1, 2, and 3 hold, the voters would not wish to move from $\bar{a}$. One voter prefers $\bar{a}$ to anything else and for any a_j different from $\bar{a}$ one voter in each pair would prefer a_j to $\bar{a}$ and the other in the pair would prefer $\bar{a}$ to a_j. In Figure 7, where the maxima for voters 1, 2, 3, 4, 5 are at points numbered 1, 2, 3, 4, 5, $\bar{a}$ would beat a_j with a majority of 2, 3, and 5 or $\bar{a}$ would beat a' with a majority of 2, 4, and 5. Furthermore, any point $a' \neq \bar{a}$ can be beaten by some other point a_j, as in Figure 7 a_j beats a' with 2, 4, and 5.

The interesting feature of Plott's conditions for equilibrium are that the likelihood of satisfying them in the real world is extremely remote. Even if they were, by some amazing chance, to be satisfied, it would still be true that even a slight change in *one* voter's preferences would disrupt the equilibrium, because it would upset the necessary pairing of opposites. For all practical purposes, therefore, we can say that, given $m > 2$ voters and $n \geqslant 2$ dimensions of judgment with continuous alternatives, equilibrium of tastes is nonexistent.

This conclusion has been extended further by translating it into the language of game theory. When a majority prefers a_h to a_j, one says a_h *dominates* a_j and one defines a *core* as the set of undominated points, which is the same as the definition of Condorcet winners. Schloss (1973) has shown that the Plott equilibrium defines a core, given that the differentiability feature is removed; and Rubinstein (1979) has shown that, generically, cores of voting games, in which only continuous preferences are assumed, are empty.

Just how devastating is the absence of equilibria has been impressively demonstrated by Richard McKelvey, whose work has been aimed at showing that, when equilibrium breaks down, it breaks down completely (McKelvey, 1976, 1978). It has been frequently supposed that, although it is extremely unlikely that an alternative can beat $n - 1$ others, still all members of a relatively small set of k alternatives (themselves in cycle) can probably all beat the remaining $n - k$ others. If this were so, then some alternative in this "top cycle" might reasonably be regarded as a satisfactory winner and disequilibrium would simply mean the absence of a clear choice among several generally preferred outcomes. McKelvey has shown, however, that given continuous utilities, the top cycle can be expected to include all possible alternatives in an n-dimensional space. In a fashion similar to Plott's, McKelvey showed with an extremely general topological model, that the conditions for an equilibrium were

1. that the indifference contour for some voter i (not restricted as to convexity or any of the usual economic assumptions) must coincide with the frontier of the set of points that can beat some point a_h.

2. that all other voters' indifference contours can be paired up in the sense that, if some points a_j and a_k are on voter i''s indifference frontier, then they must also be on at least one other voter i'''s indifference frontier.

These are, of course, very general formulations of the same kind of symmetry conditions that have been required all along, by Black, by Black and Newing, and by Plott. And they tell us about the same thing, namely, that conditions for equilibria are so restrictive as to render equilibria virtually nonexistent. Furthermore, even in the unlikely event that they should be satisfied, any single individual who is paired with another to generate equilibrium, can break it up by dissembling about his or her true preferences. Hence, not only are equilibria rare, they are also extremely fragile.

An important feature of McKelvey's conditions is that, if they are not met, then the top cycle includes all points in the policy space. This fact means that there is some way by which *any* point can beat the status quo. Suppose $\bar{a}$ is the status quo; then, if it is desired to replace $\bar{a}$ with a', there is some sequence of majority rule decisions (and often many such sequences) such that a' beats a_h, a_h beats a_j, . . . , a_k beats $\bar{a}$. Hence, any official or participant who can control the agenda can bring about the adoption of his or her desired alternative a'. But, of course, there is also a path by which $\bar{a}$ may then beat a'. So a second participant may foil the first.

A result similar to McKelvey's has been arrived at from a quite different topological model by Schofield (1978). His analysis begins with the observation that, for any point, x, in a multidimensional issue space, some indifference curve for each participant passes through x. Given these indifference curves, one can find the set of points, $P_C(x)$, which is the set of points in the neighborhood of x that are preferred to x by some winning coalition, C_k, $k = 1, 2, \ldots$. If, for the set, W, of all winning coalitions, $P_W(x)$ is such that there are some points, y, in the neighborhood of x that cannot be included in $P_W(x)$ by some path ($y\, M_{C_i}\, z, \ldots, w M_{C_k}\, x$, when C_i and C_k are any specific-winning coalitions), that is, if there are some points y that x can defeat no matter what, then there may be an equilibrium at x. If, however, all points in some arbitrary neighborhood of x can by some sequence of majority coalitions defeat x, then equilibrium at x is impossible. Furthermore, if for any particular set of individual ideal points and indifference curves, there is even just one point x for which equilibrium is impossible, then the system as a whole is cyclical. Effectively, this means that unless the individual preferences are highly similar—so that *all* winning coalitions are similar—social choices are certain to be cyclical. While this condition does not in itself indicate the likelihood of cyclical outcomes, Schofield has also shown that, if the issue space has at least as many dimensions as one more than the number of persons necessary for a minimal winning coalition, then the system is, for certain, cyclical. In legislatures most members probably have on every decision a dimension concerning the effect of the several alternatives on their chances of reelection and in electorates on all political platforms containing issues of a distributive nature ("who gets what") each participant is concerned, *inter alia*, with what he or she gets. Hence for these types of voting situations, there are at least as many dimensions as voters and disequilibrium is, therefore, certain.

In comparison with McKelvey's statement of the condition for a global cycle from $\bar{a}$ to $\bar{a}$, Schofield's theorem is a condition for a local cycle in an open neighborhood of $\bar{a}$. Practically, the difference is that McKelvey's condition guarantees instability by admitting what may seem like farfetched alternatives. (For example, the free soil issue that broke up the great Jeffersonian-Jacksonian agrarian coalition seemed to Democratic politicians, southerner and dough-face alike, to be an absurd irrelevancy because it was an issue more or less proscribed by the constitutional settlement. Nevertheless it broke up a seemingly overwhelming and persistent majority. This is the kind of event that we are assured is possible by McKelvey's theorem.) Schofield's theorem, while not excluding global cycles, assures us that local cycles can occur based presumably on only "slight" changes in the alternatives. (This is the kind of change we see in ordinary American politics where parties that appear extremely close ideologically turn each other out of office.) Hence, for most practical politics disequilibrium is assured.

The Significance of Disequilibrium

I now return to the philosophical question I raised in the beginning by asking what is the relevance for politics of the rarity and fragility of majority rule equilibria. And I start off by observing that the discoveries about majority rule probably apply to all methods of summing individual preferences. We know from Arrow's theorem that cycles cannot be avoided by any fair system, but we do not know from that fact much about the likelihood or fragility of a cyclical outcome for other methods of summation. We do know, however, that other methods of voting (e.g., positional methods like plurality voting and approval voting and point counting or electoral methods like proportional representation which are intended to make minorities win) are subject to disequilibria, manipulation, agenda control, etc., in much the same way as majority rule. It seems fairly safe to conjecture, therefore, that equilibria in other voting systems are as rare and fragile as in majority rule. And this rarity and fragility are doubtless as much features of systems based upon coopted committees as of those based on popular election. Turning to non-voting methods of summation (e.g., statements of the sense of the meeting by a speaker or the selection of alternatives by a dictator), we know that the single summarizer necessarily imposes his or her own order on the outcome. Inasmuch as equilibrium is thus achieved by suppressing alternatives that might beat the single summarizer's own choice, such an equilibrium is not the product of summation, but of force. The rebellious discontent of those whose preferred alternatives are suppressed is simply evidence that the equilibrium achieved by a dictator or single summarizer is spurious. It seems to me, therefore, that what we have learned about equilibria under majority rule applies equally well to any political society whether it uses the institutions of majority rule or some other kind of voting or merely dictatorship.

And what we have learned is simply this: Disequilibrium, or the potential that the status quo be upset, is the characteristic feature of politics.

In the nineteenth century, economics was often called the "dismal science" largely because the equilibria predicted from price theory were not palatable to those who called it dismal. In what seems to me a deeper sense, however, politics is *the* dismal science because we have learned from it that there are no fundamental equilibria to predict. In the absence of such equilibria we cannot know much about the future at all, whether it is likely to be palatable or unpalatable, and in that sense our future is subject to the tricks and accidents of the way in which questions are posed and alternatives are offered and eliminated.

Yet there are some features of social decisions that we do understand and, in the short run at least, those do grant us some prevision. Although there are not likely to be equilibria based entirely on tastes, still there are outcomes of social decision processes, those outcomes do embody some people's values, and the outcomes themselves are not wholly random and unexpected. What prevents purely random embodiments of tastes is the fact that decisions are customarily made within the framework of known rules, which are what we commonly call institutions. Since institutions certainly affect the content of decisions, we can see something of the future by specifying just what these effects are and how they are produced. Thus, despite the recent enthusiasm for studying tastes (e.g., public opinion, political culture, and the like), what we learn from recent political theory is that the particular structure of an institution is at least as likely to be predictive of socially enforced values as are the preferences of the citizen body. So the sum of the recent discoveries is to re-emphasize some of the classical heritage of political science. It is important to study constitutions simply because, if there are repetitive equilibria in social decisions, these equilibria derive at least as much from institutions as from tastes and values.

The outcome, then, of the search for equilibria of tastes is the discovery that, failing such equilibria, there must be some institutional element in the regularities (or actual equilibria) we observe. We are forced back, therefore, to the eclectic stance that political scientists have typically taken. Outcomes are, of course, partially based on tastes because some person's (not necessarily a majority of people's) tastes are embodied in outcomes. But the ways the tastes and values are brought forward for consideration, eliminated, and finally selected are controlled by the institutions. And institutions may have systematic biases in them so that they regularly produce one kind of outcome rather than another. In this sense, therefore, both institutions and tastes contribute to outcomes. To offer an example, in electoral systems it not infrequently happens that the same party or coalition of parties wins election after election. Conceivably, this stability may be caused by the fact that tastes are constant, but more often it is caused by the fact that exactly those issues likely to upset the stability of tastes are kept out of the electoral process by structures like constitutions and political parties. (An excellent example of this process is the exclusion of the issues of slavery and free soil from American national politics from the 1780s to the 1840s. The hegemony during that period of the Jefferson-Jackson Democracy, which did provide a long-term equilibrium, could exist only by suppressing the slavery issue. Once it was raised, dividing rural slaveholders from rural yeomen, the Democracy and the nation were disrupted.) What results, therefore, is an outcome based both on tastes and on the way in which some tastes are admitted to and some not admitted to the decision process.

Consequently, we cannot study simply tastes and values, but must study institutions as well. Nevertheless, as we return to the study of institutions after a generation of preoccupation with values and preferences, we do so with a deeper appreciation of the appropriate scientific program and of our opportunities and limitations as scientists. The scientific program is to explain by the application of covering laws to in particular situations how institutions generate equilibrium by systematically excluding or including certain tastes or values. As for our opportunities and limitations, we have already learned from the devleopments chronicled here that we cannot expect to find equilibria of preferences, but we may be able to find equilibria generated from a given subset of preferences by particular institutions.

In the earlier tradition of studying constitutions, it was customary to look for the centers of power in a constitutional structure—to look, that is, for who could control which portions of the political process. This is, of course, an interesting practical question for the world, because it concerns the distribution of "power." But while such distributions are a fascinating subject for ideologues and inside dopesters, they are not of much scientific interest because the idea of power is itself an inexact and probably meaningless notion (Riker, 1965). What is instead scientifically interesting is the interaction among the several participants in a system to discover the particular kinds of outcomes that are both feasible and likely, given a particular institutional arrangement.

This is the kind of study of institutions that has developed more or less unconsciously among specialists on the U.S. Congress, who are among the first political scientists to study a single institution in intensive detail. Having identified the several centers of authority in the conventional kind of constitutional analysis, they have gone on to generalize about how these centers interact in the selection of values to be incorporated into legislation. So sophisticated has this kind of inquiry become that Kenneth Shepsle, a scientist trained in both fields, has attempted to integrate it formally with the study of equilibria of preferences. Thereby he has managed to lay down an outline of what the new kind of study of institutions might typically look like (Shepsle, 1979). Shepsle distinguishes two ways in which rules and structures may impose conditions that affect the outcome of the decision-making process in a legislature, or indeed in many other kinds of decision-making bodies. One is decentralization: to divide the body into subsets which act on some issues for the whole body: e.g., committees or parties in legislatures, departments in colleges or firms or bureaucracies, etc. The other is the creation of jurisdictions: to divide up for separate consideration the dimensions of decision so that, in a policy space with m dimensions, it may be required that each dimension, $x_1, \ldots, x_m$, be considered by itself. Sometimes these two kinds of rules are combined, as when a congressional committee is given control over one feature, and only one feature, of a bill. Shepsle defines a structure-induced equilibrium as one in which, taking tastes as given, a particular arrangement of subsets of decision makers and particular assignment of jurisdictions allow for the passage of a motion that cannot be defeated by any other alternative. (Of course, an equilibrium of tastes, where, regardless of institutions, a motion can beat any other, implies an equilibrium of structure. But the converse does not hold: a structural equilibrium does not imply an equilibrium of preferences. In that sense, the notion of a structural equilibrium is narrower in meaning than an equilibrium of values or tastes.) Shepsle's main theorem is that structural equilibria exist in a committee system, provided the members' preferences can be represented by quasi-concave, continuous utility functions and the committee system operates in an m-dimensional space in such a way that each dimension is under the jurisdiction of a particular committee. (Particular assignments of jurisdiction are assumed to be protected by a germaneness rule that permits amendments in committee only on the appropriate dimension.)

The secret of this theorem is that, when social choices are made dimension by dimension, then, if an equilibrium condition (say, single-peakedness) is satisfied on one of the dimensions, some degree of stability is imposed on the whole system. Since equilibrium conditions often do exist when choice is on only one dimension, especially if the decision-making body shares cultural standards, it follows that structural equilibrium is much easier to obtain than a pure equilibrium of tastes.

But, asks Shepsle, how robust is a structural equilibrium? The answer is that, insofar as a constitutional system supplies an outcome that is not the same as outcomes that might have been obtained from simple majority rule in the system without committees, jurisdictions, etc., the losers are likely to want to change the committees and jurisdictions in the hope of winning on another day. In the end, therefore, institutions are no more than rules and rules are themselves the product of social decisions. Consequently, the rules are also not in equilibrium. One can expect that losers on a series of decisions under a particular set of rules will attempt (often successfully) to change institutions and hence the kind of decisions produced under them. In that sense rules or institutions are just more alternatives in the policy space and the status quo of one set of rules can be supplanted with another set of rules. Thus the only difference between values and institutions is that the revelation of institutional disequilibria is probably a longer process than the revelation of disequilibria of taste.

Our new sophistication about institutions, induced by our long foray into the search for equilibria of tastes, is that institutions are probably best seen as congealed tastes. We ought, I think, to be thoroughly aware that the distinction between constitutional questions and policy questions is at most one of degree of longevity. If institutions are congealed tastes and if tastes lack equilibria, then also do institutions, except for short-run events.

It is true that we can get a lot of mileage out of relatively stable institutions. If elections are zero-sum or constant-sum, then all the restrictions embodied in game theory notions of solutions of zero- or constant-sum games and all the restrictions embodied in sociological laws like the size principle are more or less permanently imposed on outcomes. Only the abolition of zero-sum or constant-sum methods of election is likely to eliminate these restrictions. Similarly, while it is easy enough to change some prisoners' dilemmas to situations with Pareto-optimal outcomes (as for example the so-called "tragedy of the commons" was solved by the enclosure of common lands into private property), still there are other apparently intractable prisoners' dilemmas (such as arms races and the extinction of species of creatures like passenger pigeons and perhaps whales). In the former cases there are governmental organizations inclusive enough to change the institutions of the dilemma. But the latter sort of institutions are likely to last for a very long time.

Nevertheless, if the non-Pareto optimal feature of an institution is sufficiently distasteful to most participants, it is possible to reconstruct institutions. Private property in land was extended to the commons to prevent the destruction of soil, and it is not impossible to imagine private property in whales. If institutions do generate an outcome in which everyone loses, it is reasonable to expect some new and less distasteful institutions—which is to say that even the most fundamental institutions lack equilibria, although it may take generations to alter them.

The sum of our new sophistication is, therefore, that political outcomes truly are unpredictable in the long run. We may have a few pretty well-verified generalizations to guide us (for example, the size principle or Duverger's law), but for the most part we live in a world that is uncertain because it lacks equilibria.

Conclusion

And this conclusion sets the problem of political science: In the long run, outcomes are the consequence not only of institutions and tastes, but also of the political skill and artistry of those who manipulate agenda, formulate and reformulate questions, generate "false" issues, etc., in order to exploit the disequilibrium of tastes for their own advantage. And just what combination of institutions, tastes, and artistry will appear in any given political system is, it seems to me, as unpredictable as poetry. But given the short-term structural and cultural constants, there is some stability, some predictability of outcomes, and the function of the science of politics is to identify these "unstable constants."

Appendix

This paper was originally presented at the meeting of the International Political Science Association, Moscow, 1979. For that occasion the concluding remarks were as follows:

Given the location of the platform for the presentation of this paper, I should conclude with the observation that political science can exist only in an open society, that is, a society with unfettered freedom of speech. Insofar as the science involves a study of values and tastes, scientists can be accurate in their predictions only if they are able to ignore official doctrine (as for example in Marxism) about the preferences and interests of groups and classes. Official doctrine may be right or wrong, but whether it is or is not right is a subject not for official decree but rather for empirical investigation, which is possible only an open society. Moreover, insofar as the science of politics involves the study of institutions, scientists must be able to examine critically the way governmental institutions operate at the highest as well as lowest levels of government. Only thus can they study the way institutions systematically bias the selection among preferences. Of course, this means that governmental secrecy, if it exists, prohibits scientific investigation of political structures.

Which of the two—official doctrine about preferences or governmental secrecy—is the more inhibiting for scientific inquiry probably varies from place to place. But I believe secrecy is more often a barrier. The scientist can often guess fairly well about tastes and preferences, but the way institutions work is extremely difficult to guess about. Consequently, if I am correct in believing that the study of tastes is not enough and that one must study institutions as well, then it follows that the new emphasis on institutions as a necessary part of the science of politics probably precludes this science in any society governed secretly.

Finally, there is another way in which the conclusions of this paper imply that political science can exist only in an open society. One important conclusion, indeed the most important conclusion, of the line of reasoning set forth in this paper is that, in the long run, nearly anything can happen in politics. Naturally this conclusion is a sharp contradiction of all philosophies of history (such as Marxism) that necessitate a belief in the existence of a determined course for the future. This belief is precisely what the discoveries recounted in this paper deny. So, if these discoveries are true—and mathematically they appear to be irrefutable—then a science of politics is incompatible with Marxism.

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Political Disequilibrium and Scientific Inquiry: A Comment on William Riker's "Implications from the Disequilibrium of Majority Rule for the Study of Institutions"

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> Politics is *the* dismal science because we have learned from it that there are no equilibria to predict. In the absence of equilibria we cannot know much about the future at all, whether it is likely to be palatable or unpalatable, and in that sense our future is subject to the tricks and accidents of the way questions are posed and the way alternatives are offered and eliminated.
>
> (Riker, 1980, p. 443)

And thus, in two sentences, I am told that I cannot attain that goal toward which my research is directed: a scientific understanding of political processes. More importantly, Riker's conclusions call into question the objectives and methodologies of most students of politics. While not all of us abide by the same paradigm, or use a similar professional argot, most of us do share, knowingly or unknowingly, a common goal: to search for political equilibria. For most of us believe that empirical regularities exist to be discovered and explained or that events are not principally determined by the idiosyncratic and unpredictable personalities of a few decision makers. Therefore, we can take comfort in the fact that Riker misconstrues the nature of equilibrium and that an appropriate understanding of this concept diminishes the overriding relevance and impact he imputes to his conclusions.

Behind Riker's argument stands a particular concept of equilibrium. Capable of being stated in many ways and even of being disguised, this concept nevertheless appears in two basic forms. First, for situations in which people act alone without explicitly coordinating their actions with others (non-cooperatively, in game-theory terms), an equilibrium corresponds to an outcome in which, *ex post*, no one person has any incentive to change his or her decisions unilaterally and to do something else. Second, for situations in which people can act in concert, with various subsets of people coordinating their actions to form "coalitions," an equilibrium corresponds to an outcome in which no coalition has the incentive or the means for unilaterally insuring an improvement in the welfare of all of its members. In game-theory terms such an equilibrium is called a core and corresponds in simple voting games to a Condorcet winner.

Scholars have long known that equilibria of either kind need not exist. Moreover, two decades of research and countless professional papers clogged with impossibility theorems point out that such equilibria are the exception rather than the rule and that "disequilibrium" cannot be precluded unless a variety of special circumstances prevail. We have also learned that, in the abstract at least, without equilibrium, an agenda setter—for example, one who can decide the order of voting—commands an extraordinary opportunity to dictate outcomes.

These are the conclusions that Riker laments. Believing that political processes do not share the straightforward stability found in abstract representations of economic markets, he infers that political scientists are disadvantaged in their "science" in contrast to economists, whose paradigm Riker has borrowed. But this claim seems less than compelling.

First, the presumed stability of markets is an abstract fiction that most economists recognize as a *theoretical* impossibility. Second, traditional definitions of equilibria may provide inadequate predictions and explanations. Third, the presence of an equilibrium in a static model does not guarantee its presence in more robust dynamic conceptualizations. Finally, disequilibrium of the traditional sort does not preclude explanation and science.

The presumed stability of markets resides principally in our abstract description of them and not necessarily in reality. That description is largely static and thereby precludes the "disequilibrium" occasioned by changing tastes and technologies. Neither economists nor anyone else knows much about tastes, how they are formed, and the laws governing their change. And, while abstract descriptions of markets might predict how the introduction of new technologies affects prices and consumption, these descriptions cannot anticipate that technology. Indeed, we can think of the firm that finds a less costly production method as a "market agenda setter"—as one who determines the set of alternatives from which we must choose—and as such possesses a powerful influence over final prices, levels of production, and future incentives for new invention.

A market's stability is also a fiction of the mathematical abstraction used to represent it. For instance, consider a market with one seller and two buyers. The usual representations of markets assign a zero marginal value to a coalition of the two buyers and the equilibrium of the core predicts that the single seller, being a monopolist, will extract all the added value from any exchange. But once the buyers learn the consequences of this situation, it seems reasonable to suppose that these buyers will form a "cartel" and negotiate as a team and on more even terms with the monopolist. Termed by some as "the problem of theory absorption," it is thus argued that the core's stability can be self-defeating in markets. A new "equilibrium" prevails, but one that is not terribly informative, for it includes all possible efficient outcomes. The monopolist's problem now is to try to disrupt the cartel or to bargain with the cartel for as advantageous an outcome on the Pareto frontier as possible. We cannot predict which situation will prevail—whether the game will involve three persons (two buyers and one seller) or two persons (a cartel and a seller) —and thus we remain uncertain about the eventual outcome.

Attempting to circumvent the theoretical problems occasioned by such possibilities, economists commonly assume that there are a great many buyers and sellers, that none individually can affect price or industry output, and that their numbers effectively preclude cartels. But, cartels do form—unions, trade, professional and farm associations, or trusts—and are designed to subvert the process of perfect competition. Cartel strategies vary, but a common one is lobbying for centralized government intervention. Such intervention, in turn, comes in many forms, including industry subsidies, benefit-in-kind transfer payments, and regulation to exclude competition.

Understanding such intervention, its genesis and its impact, is a central objective of political science. Hence, the problems Riker sees for our discipline are also common to economics. That is, because market processes sometimes resemble and are necessarily linked to government activity, an understanding of how markets function and the outcomes they produce seems impossible without also understanding political processes. If the methodologies and theoretical perspectives borrowed from economics are inadequate to comprehend political phenomena because of the absence of political equilibria, then those methodologies and perspectives must remain equally inadequate for comprehending market phenomena as well.

This cartel example also shows that the existence of equilibria does not guarantee that we can satisfactorily predict outcomes. Once the two buyers form a cartel and negotiate as one, the situation corresponds to a two-person (seller, cartel) bargaining situation in which the game-theory equilibrium termed the core consists of *all* Pareto optimal outcomes that satisfy the security levels of the individual players. Such a prediction, however, does not usually limit the number of anticipated outcomes to any reasonable degree. And, while a variety of ad hoc normative arguments and other "solutions" that seek to narrow this prediction further have been proposed for such situations, none is generally accepted.

Non-uniqueness of equilibria can bring other problems. If an equilibrium is not unique, then it may be difficult to assert that people will arrive at any such outcome. The simplest example of such a possibility is a two-person game in which each person, in ignorance of what the other does, must choose an integer between 1 and 10. If they choose integers that sum to ten, they each win, in dollars, an amount equal to the numbers they chose respectively; but, if they choose numbers that do not sum to ten, they each lose, say, $25. This game as 10 equilibrium outcomes [(1,9), (2,8), . . . , (9,1)] out of 100, but played non-cooperatively, there is no guarantee that such an outcome will prevail.

Other situations may have unique and well-defined equilibria, but their properties may be sufficiently onerous that we are convinced they will be avoided. The best example is the Prisoners' Dilemma in which two persons seem inevitably led to an outcome they unanimously prefer to avoid. While its original form corresponds to a situation in which two prisoners are locked in separate cells with a choice between confessing and not confessing, this simple game is used also to represent more complex and common situations of market failure owing to the existence of public goods and externalities. For example, choosing between polluting the environment or voluntary installation of antipollution devices can correspond to a Prisoners' Dilemma in which everyone unilaterally chooses to pollute despite unanimous agreement that installing antipollution devices is preferred.

But, if the corresponding outcomes are sufficiently onerous and inescapable, the players will soon change the game. In the case of environmental pollution, the game is moved to a higher level with government intervention to alter people's incentives—to break the dilemma—and to induce a preferred outcome. In fact, aside from issues of redistribution and equity, much of the traditional and accepted justifications for the state's existence resides in the desire to avoid those Prisoners' Dilemma situations that perfectly competitive markets are presumed to engender.

Governments, though, engender new kinds of dilemmas. With a government's usual powers to expropriate wealth, interest groups form to lobby for particularized private benefits. Economic efficiency becomes the new undersupplied public good, and, while all groups might agree jointly not to use the coercive powers of government for their particularized ends, none has any incentive unilaterally to choose another course. There is now, in fact, a growing belief that governments, in the grip of this dilemma, have grown too large and unwieldy, and that new constitutional rules such as spending limits are required to control the Prisoners' Dilemmas among interest groups and constituencies that governments engender.

All of these Prisoners' Dilemma situations, when modeled statically, have exactly the kind of unique and well-defined equilibria that Riker would prefer. But when they are viewed with any historical perspective, they portray the disequilibrium that corresponds to the tension between laissez-faire and collectivist philosophies.

To this point, these comments do not detract from Riker's concerns, and might even be interpreted to support his argument. The central point here, however, is that even if equilibria of the traditional sort exist, either in reality or in our abstractions, there remains no guarantee about the predictive and explanatory productivity of scientific inquiry; traditional definitions of equilibria may be inadequate for reasons other than existence. Yet what bothers Riker is that equilibria seem uncommon in even static theories. We cannot disagree. The theorems that demonstrate this proposition are incontrovertible. But the implications Riker draws about the presence of disequilibrium and the scientific tasks that lie before us need not follow.

First, equilibria and predictability are not synonymous. For instance, consider a simple majority voting situation in which there are 100 possible outcomes denoted O_1 through O_{100}. Suppose O_1 defeats O_2, O_2 defeats O_3, and O_3 defeats O_1 (that is, O_1, O_2, and O_3 cycle); but, individually these first three outcomes defeat all that remain. This system has no Condorcet winner nor is the social order under majority rule transitive. Nevertheless, we can predict with confidence that outcomes O_4 through O_{100} will not be chosen.

Admittedly, this kind of situation does not fuel Riker's concern. Predicting "O_1, O_2, or O_3" is to say simply that the final outcome will be in the *top cycle set,* the set of outcomes that can be reached by some finite agenda from any other outcome. What troubles Riker is that for many situations, the top cycle set may include all possibilities. Riker refers particularly to McKelvey's result that, for a broad class of spatial games, if a majority equilibrium does not exist, then under majority and other such rules, the social preference ordering is wholly intransitive: one can cycle endlessly throughout the entire alternative space. Thus, an agenda setter, confronted with a naive committee, legislature, or electorate can manipulate them to any possible outcome. Herein is the source of Riker's conclusion that outcomes are determined by the act of persuasion and the inventiveness and artfulness of our political leaders.

However, notice the important shirt in Riker's perspective, a shift from evaluating the properties of traditional kinds of equilibria—Nash equilibria, Condorcet winners and cores—to the properties of an alternative definition of a solution—the top cycle set. Riker's argument rests on the properties of but one solution hypothesis, an hypothesis that is by no means the sole candidate for resolving voting games without a core, Condorcet winner or Nash equilibrium.

Actually, Riker's argument is curious, given his research experience. Much of that experience is devoted to experimental work establishing the predictive value of a particular game-theory solution hypothesis, the Von Neumann-Morgenstern set (V-set). Briefly, the V-set is an hypothesis about what outcomes will prevail when a game (for example, a voting situation) does not have a core (majority dominant point) or when the top cycle set is large. Thus, for the three-person game in which any two people can decide how $10 is to be divided among themselves, the top cycle consists of all possible divisions, whereas the V-set predicts that two people will divide the money equally. Riker marshals considerable experimental support for this prediction.

The V-set has been largely discredited as an appropriate solution hypothesis principally because it does not exist or predicts poorly in a large class of voting games. Nonetheless, the perspective implicit in the development of the V-set is important: while personalities and abilities may dictate which particular outcome prevails, the range of possibilities can be narrowed down to some relatively small subset of the top cycle set.

A variety of alternatives to the V-set have been proposed and are now receiving close experimental and theoretical scrutiny. These include the Bargaining Set, the Competitive Solution, the Kernal, and the Defensible Set. Each alternative abides by the common belief that the absence of equilibria of the traditional sort does not preclude prediction and scientific explanation. And, while no wholly satisfactory or generally accepted solution hypothesis now exists, we should not suppose, as Riker implicitly does, that it *cannot* exist.

The scientific task before us, then, appears no different from the one Von Neumann and Morgenstern confronted: generalizing and redefining the meaning of the word "equilibrium." Twenty years of research compel us to conclude that traditional definitions of equilibria are unsatisfactory, or at least that they are not sufficiently general. Several such generalizations, such as the top cycle set, have been attempted but none possesses the full range of desired properties. The inadequacies of these generalizations, however, are only now beginning to be appreciated, and as the need for more general definitions becomes apparent we can suppose that considerably more effort will be directed at this scientific task. Disregarding his pessimism about the productivity of abstract theory, Riker's essay nonetheless highlights the necessity for further fundamental theorizing in the areas of decision theory, game theory, and the rational choice paradigm.

In summary, Riker incorrectly equates the possibility and fruitfulness of scientific inquiry with some notions of equilibria that are themselves subject to refinement and scientific advancement. Thus, reflecting on his argument and on the near countable infinity of impossibility theorems and negative results that form its basis, I arrive at a conclusion that contrasts sharply with his. I do not view these results as proving something unsavory or even disturbing about democratic processes in particular and political processes in general. Rather, they tell us that such processes do not operate in trivial, straightforward ways; that theorizing about them requires developing new concepts, and that the optimism of the past over the ease with which the economists' paradigm could be transplanted into politics must give way to the realization that political scientists themselves must contribute to the development of that paradigm.

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An Altimeter for Mr. Escher's Stairway: A Comment on William H. Riker's "Implications from the Disequilibrium of Majority Rule for the Study of Institutions"

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> The real world of values is inconsistent; that is to say, it is made up of antagonistic elements. To grant them full recognition simultaneously is impossible, yet each demands total acceptance. This is not a matter of logical contradictions, because values are not theoretical theses. It is a contradiction which lies at the heart of human behavior.
>
> Leszek Kolakowski (1968, p. 216)

> Politics is *the* dismal science because we have learned from it that there are no equilibria to predict. In the absence of equilibria we cannot know much about the future at all, whether it is likely to be palatable or unpalatable, and in that sense our future is subject to tricks and accidents of the way alternatives are offered and eliminated.
>
> William H. Riker (1980, p. 443)

William Riker's article explains a central thesis of social choice theory with its author's characteristic vigor. The initial notion, first put forward some 195 years ago in Condorcet's *Essai* (1785) is this: Given three or more alternatives (say, laws or candidates) and three or more voters, majorities may march in circles even while individuals do not. The contemporary literature presents an essentially two-sided extension of Condorcet's little discovery: (i) that the Condorcet paradox can be avoided only at cost of violating some other reasonable-sounding axioms for social choice (viz., Arrow's theorem), and (ii) that under majority rule itself, cyclic majorities will be common, often large, generally without a single alternative immune to the process of cyclic dominance. This second range of findings, built up by Kramer, Plott, Fishburn, Bell, McKelvey, Schofield, and many others, tells us that the Condorcet paradox is no fluke, and therefore is not the dismissible "phantom" which Gordon Tullock used to make it out to be. It must be integrated with, not banished from, our understanding of political theory. This is what Riker tells us, and I agree. The question is to see why we should *care* about transitive consistency in liberal democratic (or any other) political theory.

Why Transitive Consistency?

To the untutored eye, the underlying idea is not prepossessing. Why should we care about the possibility—even the overwhelming likelihood—that majorities may go marching around in circles? What, in other words, justifies transitivity itself: If x is better than y, and y is better than z, *why should* x be better than z?

Surely there are some aspects of life in which transitive consistency would be rejected by persons not certifiably irrational. Imagine, for example, the following announcement to a crowded stadium:

> Cal Tech defeated Carnegie-Mellon on September 12, Carnegie-Mellon defeated Harvard on September 19, Harvard defeated Minnesota on September 21, and Minnesota defeated Texas on September 28: Therefore Cal Tech has been declared the winner of today's game with Texas, which will not be played lest a violation of transitive consistency occur.

One would need *guns* to justify this reasoning.

Or consider Leo Strauss' case against rational consistency in the thought of Max Weber (1950, p. 47):

> I must act rationally: I must be honest with myself, I must be consistent in my adherence to my fundamental objectives, and I must rationally choose the means required by my ends. But why? What difference can this still make after we have been reduced to a condition in which the maxims of the heartless voluptuary as well as those of the sentimental philistine have to be regarded as no less defensible than those of the idealist, of the gentleman, or of the saint? We cannot take seriously this belated insistence on responsibility and sanity, this inconsistent concern with consistency, this irrational praise of rationality.

What this suggests for the present case is that the formal vacuity of transitive consistency—aimed at ciphers like "x" or "a_j"—may miss what is rationally defensible (even what is consistent)[1] by placing all laws, all leaders, all policies under its blind exaction, and doing so blindly.

Coming back within the horizon of Riker's analysis, please hear Robert Dahl asserting the *irrationality* of the demand for transitivity within a democratic framework (1956, p. 42, italics added):

> Because Arrow . . . assumes "transitivity of collective choice" as a criterion of rational social action, it is worth noting that under almost any theory of democratic politics . . . the *requirement of transitivity would be irrational* in a great many types of collective choices. By "transitivity" we mean, on the analogy of inequalities in mathematics, that if an individual prefers x to y and y to z he must also prefer x to z—at least, if he is to behave rationally. But whatever the case may be with individual choices—and even here the requirement is somewhat tendentious—clearly it would lead to *irrational* results in a democracy to require transition choices. For example, along 101 individuals assume that
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> 1 individual prefers x to y, and y to z
> 50 individuals prefer z to x, and x to y
> 50 individuals prefer y to z, and z to x.
>
> Then 51 prefer x to y, 51 prefer y to z. If we assume transitivity in collective choice, it would follow that a majority of at least 51 prefers x to z also. But in fact, 100 individuals prefer z to x. And *the requirement of transitivity would produce the anomalous result that the preferences of the singular eccentric would be translated into public policy despite the fact that 100 individuals prefer the opposite policy.*

Now Riker and his colleagues supply a kind of answer to this heterodoxy, namely, that intransitivity makes majority rule not simply inconsistent but also *unmajoritarian.* Indeed, the final conclusion of his essay is that "In the long run, outcomes are the consequence of the political skills and artistry of those who manipulate the agenda, formulate and reformulate questions, generate 'false' issues, etc., in order to exploit the disequilibrium of tastes for their own advantage" (1980, p. 445).[2] In short, majorities become the means of minority rule.

But then Dahl (or, better, a Marxist critic) can remind us that the undemocratic forces which *really* limit majoritarianism come before preferences form, if ever they do form, and are at most complicated by the disequilibriating effects of intransitivity. This is not to dismiss Riker's point, or to imply that transitivity is meaningless, but it does suggest that its importance lies elsewhere, in a larger argument of which it is only part.

Two Liberal Values

The central concern of all liberal thought is the promotion of *rights* for persons. Yet, at least since Condorcet's era, liberal thinkers have given two utterly different accounts for the *value* of rights. Neither view is singular or simple, but both are coherent enough to serve our small purpose. The first view, held by figures as different as Paine and Kant, treats rights as valuable in themselves, no matter what their consequences may turn out to be. It is not that rights tend to promote some further good, but that rights are right. A second view, taken by Bentham, Mill, Pareto, Hayek and many others, claims that rights have good consequences—for the discovery of truth (Mill), or for economic efficiency (Pareto), or for framing felicitous laws (Bentham). This second view then goes on to imbed these good consequences in a *utilitarian* theory of value, so that we are asked to think that rights promote the maximization of utility in society, and are to be respected for this reason.

This two-sided theory of value perpetually threatens to come apart when either (i) rights fail to promote utility, or (ii) the efficient promotion of utility entails a violation of rights. It is not, therefore, entirely surprising that the most revered works of liberalism have included those which forge an analytical link between rights and utility, so that we may rule out embarrassments (i) and (ii) as defined above.

While Mill's *On Liberty* (1849) presents a complex and deeply problematic instance of such joinery, the most spectacular attainment of this sort is not a single work but a tradition and discipline, neoclassical economics. No one book can be said to contain the core of neoclassicism, although my own vote for the single most essential work would go to Pareto's *Manual of Political Economy* (1971). For present purposes, we need not explore the neoclassicists very extensively, but need only consider two conceptions—property rights and Pareto-style efficiency—as they are related to one another in economic theory. Here in the simplest terms are the essentials:

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[1] We might thus find ourselves consistently choosing what is absurdly inconsistent, if it were observed that x was preferable to y, and y preferred to z, therefore x preferred to z, where:

x = giving everyone an income above the national average,
y = giving everyone an income equal to the national average,
z = giving no one an income on which he or she will starve.

In brief, the consistent selection of an inconsistent policy.

[2] Notice, incidentally, that Riker assumes that political manipulators care mainly about policies, while in fact the incentives they face may induce them to care very little indeed about actual policy outcomes. See, for instance, Mayhew (1976).

(1) *Property rights* mean the authority to dictate the use of what one owns, and to sell it as one chooses, free from constraint or coercion by others.

(2) The *Pareto Principle* tells us: (a) that one social state x is Pareto-superior to another social state y if and only if some persons prefer x to y and none prefer y to x, and (b) a social state x is Pareto optimal if no other state y fulfills criterion (a) in respect to it.

The innocuous-seeming Pareto principle is the version of utilitarianism which these economists accept, and upon which their rights-utility synthesis is based. Its main feature is that no cardinal, interpersonal comparisons of utility, like those imagined by Bentham, are required. This last, the stricture against "inter-personal comparisons," is a methodological point pregnant with philosophical and ideological significance.[3]

Neoclassical economics is to be sure a complex doctrine, and it is currently perhaps less simple and more problematic than at any time in our century. I therefore neglect a great deal in saying so little, but only a little needs saying for our immediate purpose. It is this: Property rights are the sine qua non of the Pareto principle. These rights promote utility in Pareto's sense at two distinct levels. They provide, first, an epistemology of utility by revealing preferences through behavior. The utility of a transaction for a given person need not be measured by politicians or scientists, for it is measured by the agents themselves: If I give you alpha in exchange for beta, then my utility is increased; if you accept alpha for beta, yours is also increased. Because we have property rights, and are thus thought to be free of coercion and compulsion, this indication of increased utility is, the doctrine assumes, to be taken as valid. At a second level, property rights provide the practical rubric for actually effecting such gains of utility. Every market transaction actually consummated is, in theory, the implementation of precept (2a), for the post-transaction outcome is preferred by both traders to the pre-transaction outcome. If and when transactions occur, Pareto-superior arrangements displace Pareto-inferior ones. We thus can think of the market as a system of innumerable little liberal polities, each lasting only as long as its partners find mutual gain from trade, each evanescing at the merest rejection of a price or lot of goods. Each such polity is created by right of property, and each may be terminated by the same right. Utility is promoted thanks to the existence of these rights, and we know it to be a promotion of utility because of these rights. The two faces of liberal value are in this way welded together as means to end, end to means, inseparable faces of the liberal market itself.[4]

It would, therefore, be merely a cruel illusion if the market went round and round in circles, never approaching the state of optimality posited by precept (2b). If the market began with distribution x, then passed by a vast series of transactions through distributions y and z, what an awful embarrassment it would be to discover that we had traded our way back where we began, to x. What a monstrous failure and fraud would the precept of Pareto-superiority now seem, for it suggests that society is getting to better and better places—superior ones with more and more social utility—and yet we have come back to where we began. If this cyclic, disequilibriating process occurred because people changed their tastes, that would be tolerable. But if it happened for any other reason, and optimality was never approached, that would make markets like one of Escher's stairways leading always up yet always coming back down to its own foundation.[5] Most of all, it would spoil the reconciliation of rights and utility. *The idea beneath all this is our friend transitivity, and its importance is to underwrite the rights-utility bond.*[6] Transitivity works as an altimeter which assures that the stairway leads always up, to loftier preference levels, to greater utility. It thus assures that the rights-utility bond is self-consistent. A longer article would pause to criticize and observe the nuances of this point, a point somewhat less straightforward than it may seem. But for now, our job is simply to see how *badly* majority rule in government compares with the market scheme as it is *imagined* in this simple digest of neoclassical thought.

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[3] See my "Limits of Consensual Decision" (1975).

[4] In this connection, I cannot resist noting that Riker is quite wrong to brand Marx "absurd." Here is Marx on the sort of thinking we are summarizing here, which he mocks as

> a very Eden of the innate rights of man. There alone rule Freedom, Equality, Property, and Bentham. Freedom, because both buyer and seller of a commodity . . . are constrained only by their own free will. . . . Equality, because each enters into relation with the other . . . and they exchange equivalent for equivalent. Property, because each disposes only of what is his own. And Bentham, because each looks only to himself. . . . Each looks to himself only, and no one troubles himself about the rest, and just because they do so, do they all, in accordance with the pre-established harmony of things, or under the auspices of an all-shrewd providence, work together to their mutual advantage, for the common weal and in the interest of all (1906, Vol. 1, p. 194).

The same two-sided account of value appears again when we turn to the liberal state and majoritarianism. Here is a classic rights-in-themselves argument from Tom Paine's *Dissertation on First Principles of Government* (1953, p. 165):

> The right of voting for representatives is the primary right by which other rights are protected. To take away this is to reduce a man to slavery, for slavery consists in being subject to the will of another, and he that has not a vote is . . . in this case. The proposal, therefore, to disfranchise any class of men is as criminal as the proposal to take away property.

Rights may be seen as protecting other rights, but rights are themselves what must be valued. This stands in stark contrast to the theory of constitutional order, of collective choice, proposed by Buchanan and Tullock, Downs, Riker, and the public choice school generally. Here, utilitarian tradesmen take center stage, as in this metaphor:

> An acceptable theory of collective choice can perhaps do something similar in pointing the way toward those rules for choicemaking, the constitution, under which the activities of political tradesmen can similarly be reconciled with the interests of all members of the social group (Buchanan and Tullock, 1962, p. 23).

The idea is that the value of constitutional arrangements, including voting rights, are reducible to their consequences as measured in utility. Now it would be a fine and useful theory which could link the rights-qua-rights and the rights-utility views of voting.

Escher's Stairway

Let us therefore imagine that Riker and all his associates right back to Condorcet are flatly wrong, and indeed that we can forge a link between a further pair of ideas:

(1′) *Voting rights* mean the authority to express one's judgment of state policy and to have those judgments counted under the principle of majority rule.

(2′) *Majoritarian Analogue of Pareto Principle* tells us: (a) that every political outcome x preferred by more rather than fewer voters to its alternative y is majority-superior, and (b) political outcome x is majority-optimal if no other outcome y fulfills criterion (a) in respect to it.

Now this ill-shaped principle (2′) is tailored to fit majority voting rights just as the Pareto principle x fits property rights. It succeeds in the small fact that (2a′) follows majority rule (1′) as neatly as (2a) follows market exchange (1). That is, however, only a small blessing. For (2a′) has no definite relation to the principle of utility maximization, and is in this way unsuited to the tasks accomplished by (2). For, if the minority loses more utility than the majority gains, then majority-preference may actually diminish total utility. This is a first, and important fissure in our little welding job. Dahl puts it very nicely (1956, p. 90):

> By making "most preferred" equivalent to "preferred by the most" we deliberately bypass a crucial problem: What if the minority prefers its alternative much more passionately than the majority prefers a contrary alternative? Does the majority principle still make sense?

If one relies upon a link between voting rights and utilitarianism, the answer is either a firm "no" or an equivocal "maybe"—which is quite enough to cause us to wonder whether majority rule is not an Escher staircase, and whether we do not need an altimeter which assures our upward progress.[7]

We turn now to (2b′) which defines a "majority-optimum" as a policy against which no further policy wins a majority.[8] This is exactly what Riker and his associates tell us cannot be expected, for the operation of (2a′) would lead to the discovery of an outcome meeting (2b′) only if transitive consistency governed its behavior. Only then could each binary application of majority rule be a knowable step up in utility space, and only then could we make the rights-utility link which is so characteristically important in liberal thought. Only then would we possess an altimeter so as to treat a staircase in confident knowledge that none of Mr. Escher's tricks would take us back where we began. What we have learned from the work discussed in Riker's article is that this altimeter—transitive consistency—will not give us the sequence of increasing altitude readings which we need.

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[5] For a discussion of Escher in a broad and speculative essay of great merit, see Hofstadter (1979).

[6] It must be admitted that the Pareto principle itself would violate transitivity if the judgments it deems undecidable were declared indifferent.

[7] It may actually be conjectured that this point partly *explains* the intransitivity of majority rule. Imagine a cardinal, interpersonal criterion of utility and hypothesize *any* decision rule which never chooses less total utility over more total utility: Could such a scheme ever violate transitivity? It could not, and could not do so for the same reason that a grocer's scale cannot. Majority rule is not such a scheme and thus does assure transitivity.

[8] Criterion 2b′ corresponds to the solution pro posed in Condorcet's *Essai*, viz., of points to what is

I think that is reason enough to grant the importance of these results, but I do not think it provides a reason to think we have arrived at a resting point in the theory of constitutions. I think it tells us that utilitarianism, including its Pareto version, is simply one among many criteria of choice which men and women may apply *within* a democratic order. Though John Plamenatz (1973, esp. p. 184) was evidently unwilling to explore the literature Riker considers, he was driving thought in a similar direction when he wrote that

> democracy is a matter of rights and obligations.... Whoever seeks to explain it, no matter how much his purpose is merely to explain and not also to justify, cannot avoid a kind of exercise which is more properly called philosophy than science.... Democracy can neither be explained nor justified as a political system that maximized the satisfaction of wants (or the achievement of goals) better than other systems do (1973, p. 181; see also Hart, 1979).

An understanding of majority rule, of democracy, of liberalism which does without utilitarianism, and which does more than merely assert that rights are right, must travel in a more mysterious space, must walk up odder stairs, and must employ a more intricate altimeter than transitive consistency.

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now called a "Condorcet Winner" (which, we now know, may not exist).

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A Reply to Ordeshook and Rae

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Both my critics agree that, under majority rule, individual values are likely to be in social disequilibrium—barring such rare events as unanimity on a complete ordering of all possible alternatives, etc. From this admitted potentiality of disequilibrium, two kinds of inferences can be drawn:

1. In the realm of political philosophy, the inference that social decisions under majority rule can not usually be defended as logically coherent or as the work of some anthropomorphized entity like the "society" or "the people." Given that a winning platform or motion or candidate exists, the fact of disequilibrium means that, at the time of the choice of the winner, there existed potentially a platform or motion or candidate that could beat the winner. Furthermore, potentially another alternative could beat the potential victor over the actual winner, and so forth around a cycle. This fact in turn means that the products of majority rule are probably seldom defensible as consistent or as the "true" choice of the voting body. Indeed, with a slightly different turn of events, some other alternative could have been the choice of a (differently composed) majority, though not necessarily a more coherent choice than the alternative actually chosen.
2. In the realm of political theory (i.e., description as distinct from prescription), the inference that social decisions cannot be predicted simply from a knowledge of individual orderings of values. Given a set of individual tastes, what comes out of majority rule is a function not only of the tastes of persons, but also of the political institutions surrounding the process of voting, of the skill with which individuals manipulate the selection of alternatives and the statement of issues, and indeed even of the intelligence and character of the voters. Consequently, the prediction of social outcomes is rendered difficult.

The Philosophical Problem

My article now under discussion was limited entirely to the inference in the realm of political theory or description and had nothing to do with political philosophy and the defense or criticism of democracy. Indeed, insofar as something remotely like majority rule exists in all societies with even the most muffled forms of consent, my discussion was intended to deal with a quite general description of politics. It is, therefore, quite a surprise to find Douglas Rae (1980) discussing my article as if it dealt with the inference in the realm of political philosophy. While I believe that the latter inference is worth considerable attention, and I have in fact discussed it at length in a paper delivered at the 1978 meeting of the American Political Science Association (where it was commented on by Rae) and in a forthcoming book, *Liberalism against Populism* (1980), I did not discuss it here because I sought to focus attention on the theoretical question. Since, however, Rae has raised the philosophical question, I feel constrained to make a few remarks about it.

I point out first that, whatever may be the need of classical economic liberalism to reconcile rights and efficiency, liberal democracy has never faced that problem because it has never been concerned with social utility as set forth in Rae's principle (2'b). (It is only populists from Rousseau to contemporary social democrats who anthropomorphize a majority which is to have its utility maximized.) Rather, liberal democracy (as explicated by, for example, Madison) is aimed solely at protecting rights, and in that sense I can quite agree with the remark of Plamenatz, quoted by Rae, that democracy has nothing in particular to do with satisfying wants. Indeed, I have utterly failed to make my position clear to Rae if he believes it is utilitarian. My position is indeed based on methodological individualism and on the assumption that individual persons choose rationally, but nevertheless it is not utilitarian, and I can find as much amusement as Rae did in his well-known quotation from Marx on the utilitarians, who from Bentham and Mill to Rawls and Nader have consistently subordinated humane values to some arbitrary and imposed virtues they prefer. (Just for the record and in response to Rae's note 4 (p. 453), I called Marx "absurd" (in the precise sense of "plainly irrational"), not because he satirized utilitarianism—in words, as he said, to give the bourgeoisie cause to remember his carbuncles—but because he reduced the complexity of politics to a few economic institutions, thereby reducing human beings to atoms maneuvered by economic forces, one-dimensional people in a one-dimensional world. One of the great virtues of the literature here reviewed is that it rescues the humanity of mankind from the slough of technological determinism in which Marx tried, misanthropically, to submerge it.)

My more significant failure in conveying my position to Rae is revealed in his belief that my concern is that majority rule is not likely to be consistent. While it is certainly the case, as he agrees, that disequilibrium implies inconsistency, the main problem for democracy is not inconsistency but the manipulability of majority rule outcomes. Given manipulability, it is likely that outcomes will never have any meaning, that they will always involve strange bedfellows, and that, even if one sacrifices consistency (as I am perfectly willing to do), there is simply no possible way to interject meaning into majority rule decision, that is, no "more intricate altimeter" can exist. It does not follow, however, that democracy is without value. Rather, it has a different kind of value: the protection of rights by means of popular vetoes over officials, as explained originally by Madison and latterly in my forthcoming *Liberalism against Populism* (1980). A democracy of that kind is, however, wholly incompatible with imposed moral standards so that people like C. B. MacPherson or John Rawls cannot possibly be democrats in the one sense of the word that remains after the revelations of the literature I have surveyed.

The Theoretical Problem

Turning now to the inference from disequilibrium in the realm of political theory, which was entirely the subject of my survey of literature and which is indeed the subject that Peter Ordeshook (1980) addresses, I think this format of criticism and response is useful because it allows for a clarification of issues.

Unfortunately, Ordeshook has misread my emphasis. For example, he seems to think that I am attempting to import economic models or economic notions of equilibrium into politics. Yet it is precisely that importation I am objecting to. My whole argument is directed toward a renewal of political scientists' interests in the study of institutions. This study has to some degree been neglected, I argue, because of the influence of the economic model which, as I observed, overemphasizes tastes. Consequently, I agree for the most part with Ordeshook's conclusion, which, however, he believes differs from mine, that "the optimism of the past over the ease with which the economists' paradigm could be transplanted into politics must give way to the realization that political scientists themselves must contribute to the development of that paradigm." My only disagreement with this conclusion is that it does not go far enough: the paradigm political scientists ought to develop is uniquely political and it is not their duty simply to develop "that paradigm" from economics.

There are a number of other details in Ordeshook's remarks that indicate that he believes our opinions differ, when in fact we have extremely similar views. Let me make it clear that simply because I have pointed out that the form of issues (often an individual contribution) influences outcomes in the same way as institutions and individual values, it does not follow that I have denied the possibility of a science of politics. That an equilibrium of individual values is extremely unlikely means that one cannot predict political outcomes simply from a knowledge of those values. It does not mean that there is no science of politics. Indeed, the whole purpose of my article is to point out precisely what that science consists of. I do not deny but celebrate the possibility of a scientific understanding of political *processes*. What I deny is, for the present, the possibility of a general scientific prediction of outcomes. As I emphasized throughout my review, it is exactly empirical regularities of process which I called "unstable constants" and which are the interesting features of institutions, that we ought to be studying, now that we know for certain that particular outcomes of interlocking tastes are not themselves predictable.

As Ordeshook points out, this is one kind of empirical work that both he and I have done in studying particular solution concepts for games. These solution concepts exist in terms of a set of rules—which are institutions. The fact that a particular set of rules, such as the kind of communication allowed, the method of voting, the kind of exchanges allowed, etc., influences the kinds of outcomes in the same way as do the tastes of the participants is a scientific discovery of great value, though one ought not to confuse it—as Ordeshook does—with general theory. It is even more valuable when one can state in just what ways the institutions (rules) do in themselves influence outcomes. Hence the comparative study of what combination of tastes and rules lead to outcomes described by such solution concepts as the V-set, the bargaining set, the competitive solution and others which Ordeshook lists is one of the kinds of scientific investigation that I regard as both possible and important. Precisely like Ordeshook, I think that it is the absence of what he calls equilibria of the "traditional" sort and what I call equilibria of tastes—and what probably should be called "general" equilibria—that leads us to investigate these processes. It was because I believe that these things are what make up the unique subject matter of political science (as distinct from, for example, economics) that I wrote this review of political theory. Far from wishing to transplant economic theory to politics (as, so I infer from the fourth paragraph and then the last sentence of his comment, Ordeshook seems to think I wish), I wrote and write to emancipate political theorists from their fascination with the economists' equilibria of tastes and to urge them to study political institutions.

It may seem from these remarks that I regard my position and Ordeshook's as identical even though he does not. But in fact there are important differences between us:

For one thing, "redefining" the notion of equilibrium, as Ordeshook proposes, into what he mistakenly believes will be general, is not likely to improve the science because we now know that general equilibrium is impossible. If equilibria of tastes are indeed rare and fragile, then redefining "equilibrium" will not produce equilibrium. To believe so is like believing one can cure cancer by redefining it as a state of health. Ordeshook's main criticism of my article is that I have "misconstrued" equilibrium. I think he is the one who misconstrues. By assuming he can redefine equilibrium into existence, he is attempting to substitute a word for the thing. But redefinition will get us nowhere. A more sensible route, I think, is to specify special equilibria for particular institutions, as, for example, Ordeshook and McKelvey have themselves done with the competitive solution—for which the particular institutional feature is the absence of side-payments. If every set of institutional arrangements has a different kind of solution (or "structural equilibrium," in Shepsle's words), we may often have problems deciding which game is going on in the real world, but at least we will be working toward the understanding of institutions that I think we can increasingly achieve.

Incidentally, Ordeshook calls my use of "equilibrium" the "traditional" usage and also says it is "peculiar." How did he get himself into this contradiction, which consumes and vitiates most of his essay? The answer is, I believe, that he has systematically confused the structural or institutional equilibria of game solutions—which, though abstract, are still special cases—with the general equilibrium of tastes.

For another thing, I think Ordeshook and I disagree rather sharply about how fast the science of politics is progressing. For the active theorist and investigator, each step forward toward the identification of an empirical regularity is a triumph. But identification of such regularities in theory and the laboratory is a long way from encompassing the political reality of the larger world (see my article, 1977). I suppose this disagreement is largely a matter of temperament. His position might be described as enthusiasm while mine is a more cautious optimism.

The really important difference between us, however, lies in our notions of what kind of science the study of politics is. A science can describe either process or outcome or both. The nature physical sciences, and even micro-economics, for example, do both. Some sciences, in their beginnings (for example, the study of evolution prior to recombinant DNA) emphasize process at the expense of outcomes. Other sciences, perhaps descriptive geology (before plate tectonics), emphasize outcomes at the expense of process. I think political science is now in the stage of emphasizing process very much like biology in the first part of this century. Ordeshook believes our science is much more mature and can handle both process and outcomes. Only time and future developments will show which one of us is right about the state we are in now, although assuming there is progress no one in the future will care very much about this dispute—which will then be ancient history.

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COMMUNICATIONS

On Articles

Comment on Nelson and on Baloyra (Vol. 73, December 1979, pp. 1024–38, 987–1002)

To the Editor:

Two articles in the December, 1979 *Review* illustrate a widespread problem in the presentation of results from sample surveys. Simply put, essential elements of survey procedures are too frequently omitted from published analyses.

In "Ethnicity and Socioeconomic Status as Sources of Participation," the only information Dale Nelson provides about the source of his data is that it is a survey conducted in northern Manhattan by the Bureau of Applied Social Research. There is no indication of what "northern Manhattan" refers to (it is not a common term), how the sample was drawn, who did the interviewing (e.g., were they members of a single ethnic group?), nor any of the other details necessary to evaluate a survey.

One consequence of this inattention to the methods used to collect data is demonstrated by Nelson's assumption that his findings are applicable to New York City, exactly one paragraph after telling us that the sample comes only from northern Manhattan. Given the subject of the analysis, this is potentially serious. The political character of ethnic groups in New York City most likely varies by neighborhood. To take but one example, the "participant culture" of Jews in Brighton Beach in Brooklyn is probably quite different from that of Jews on the Upper West Side in Manhattan.

The fact that the data presented in "Ethnicity and Socioeconomic Status" do not represent New York City as a whole can be gleaned from the article. But the absence of any other information about the survey makes it impossible to assess the extent to which the results represent even "northern Manhattan." How were the blocks in the study chosen? Manhattan typically produces among the highest non-response rates in the entire country. How high was non-response in this study? Did it lead to an over-representation of older, retired people? More generally, what are the grounds for believing that the sample is representative of any population?

There is a similar paucity of detail about the survey that is the basis of Enrique Baloyra's "Criticism, Cynicism, and Political Evaluation," although in this case there is at least a footnote indicating that details about research design may be found in a book, based on the same data, co-authored by Baloyra. The problem here is twofold. First, the cited book omits important information, such as response rate. Second, some of the information in the book should have been reported in the article, for without it informed evaluation of the article is not possible.

For example, with the exception of the dependent variables, there is no description of how variables were measured. Consider the variable labeled "stratification" in the path diagrams. An incomplete list of the terms used in the text to refer to the variable includes "social circumstances," "class ethos," "socioeconomic factor," "social standing," and "class experiences," but the reader interested in something more concrete, for instance, whether educational attainment is included in the construction of the measure (as the book reports it is), is left in the dark.

Or, consider the variable called "political agility or level of information." How many readers assumed that this was the total number of times a respondent said "don't know" to questions in the interview (most of which were attitudinal), as is reported in the book? My point here is not to criticize the decisions made about how constructs were to be measured, but to argue that the reader cannot sensibly interpret the analysis without knowing what these decisions were.

The past few years have seen the development of standards for the reporting of survey results in the mass media (see, for example, the American Association for Public Opinion Research code). Similar standards should apply to scholarly journals. Reviewers and editors should insist that at least the following information be present in articles that draw on survey data:

1. A description of the population sampled and details of how the sample was drawn sufficient to permit fairly exact replication;
2. Response rate (for quota designs, the number of refusals);
3. Interviewer characteristics;
4. Exact question wordings.

With the possible exception of data whose documentation is widely available through ICPSR, it should not be sufficient to reference another publication for this information. Adoption of such standards will make it easier for readers to evaluate published work, as well as improve the work itself by increasing authors' sensitivity to what are critical factors in the conduct of research.

STANLEY PRESSER

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Editor's Note: The points raised by Stanley Presser are well taken. In the future, manuscripts relying on data drawn from sample surveys not available from the ICPSR must include an appendix with the information cited in Presser's letter.

Comment on Franklin and Mughan (Vol. 72, June 1978, pp. 523–34)

TO THE EDITOR:

Changes in the level of class voting over time confront the political analyst with an unusually difficult problem, to wit, specifying what it is about class which produced the change. "Social class" is not a simple variable with an obvious operationalization (e.g., income in £), but a "composite" or "global" variable (Rosenberg, 1968, Ch. 2) encapsulating numerous properties—occupation, education, income, life style—often themselves equally complex. Several attempts have been made recently to enumerate the components of class and to assign each its appropriate portion of causal responsibility for the observed decline in British class voting. Franklin and Mughan (1978) provide an insightful review of these analyses, arguing that earlier estimates of the impact of certain components are invalid, and they propose an alternative estimation method based on causal modeling.

Some earlier studies suggested, for instance, that occupation had become nearly trivial as a predictor of class voting. Franklin and Mughan claim to show that this conclusion is in error. On completing the Franklin and Mughan article, the reader unfamiliar with the more arcane aspects of class voting studies might have wondered why they needed to mount such a quest in defense of the occupation variable. Would any sensible person have proposed dropping occupation from a theory of class voting? The surprising answer to this question is "yes," and the reason lies in the composite nature of the social class concept. With a number of alternative measures of class in a prediction equation, the regression coefficient for occupation falls to near zero (cf. Rose, 1974, p. 48). The authors show that two ways of calculating this coefficient can be misleading. This note points out that their alternative is subject to the same underlying difficulty. The argument is sufficiently general to support the conclusion that the simple application of regression procedures (including the stage-wise approach underlying AID, stepwide regression, and the causal modeling approach proposed by Franklin and Mughan) usually will not produce coefficients suitable for comparing the unique explanatory contributions of the component elements of a global construct.

The statistical problem inhibiting the estimation of unique contributions of correlated independent variables is usually phrased in terms of multicollinearity, and nearly as often ignored, on the ground that the symptoms of multicollinearity (high standard errors, suggesting unreliable coefficient estimates) generally are absent unless correlations among independent variables reach .7 or .8. Survey researchers, in the fortunate position of never glimpsing coefficients even near this magnitude, feel exempt from such dangers. This is a mistake. Correlations of much smaller magnitude, especially among members of a "set" of independent variables (e.g., components of a global variate) can produce errors in coefficients which are not accompanied by corresponding inflation of the estimated standard errors for those coefficients (Brody and Verba, 1972, Appendix H; Farrar and Glauber, 1967).

An intuitively appealing illustration of this problem can be motivated by considering the logic of partialing by means of control variables. The notion behind the partial coefficients produced by multiple regression is the allocation of causal force among the various sources of the dependent phenomenon. The criterion for significance of any given variable is the ability of its regression coefficient to withstand attempts to partial out its impact on the dependent by introducing successive control variables. These control variables have two characteristics. First, their use is justified on theoretical grounds: they are "test variables" (Lazarsfeld, 1955, pp. 115–25) whose purpose is the evaluation of alternative explanations. Second, control variables are independent of concepts already included in the equation.

This second point puts the matter of theoretical justification in terms of conceptual distinctness. For some purposes, occupation, education, and tenancy are distinct variable (cf. Kohn, 1976, for instance), but for an investigation of the sources of class voting they are components or alternative indicators of social class (cf. Jackman and Jackman, 1973). As such, they each make some contribution to the class-vote relationship and, if used together, *divide that zero-order relation among themselves.* This, in verbal form, is the key to Franklin's and Mughan's criticism of stepwise and stage-wise regression (because the cut points in allocating the variance are arbitrary), but it is also a deficiency of their own analysis. For instance, say that a perfect measure of class could be developed and would show the class-vote correlation in Britain to be .50. No available measures achieve this optimum, but a number of alternative indicators of social class have respectable zero-order relations with the vote. Combined into a single index, if there are no canceling effects among the components, the correlation between the vote and the class index may approach .50 (cf. Sullivan, 1971). Used as separate control variables in a single regression (as in Franklin and Mughan, 1978, p. 531), these alternative indicators must share a constant portion of the variance (R^2=.25). This portion might be allocated equally, producing a number of nearly identical but small coefficients, or it might be captured by one of the components to the detriment of the rest. The results in the latter case would appear to show that an obvious indicator of SES had lost its power to explain the vote, over a period when its bivariate relation with the dependent variable was essentially constant, and its correlation with other components in the set changed only marginally.

How the global variable's share of the variance is apportioned among its components is a function of (a) the number of component variables (the more the sharers, the smaller the shares, *cet. par.*) and (b) the strength and pattern of relations among the components. The correlations among components need not be at the .7 to .8 level: difficulties often arise with intercorrelations around .5. (For social class variables in Britain, they range between .45 and .65. Cf. Treiman and Terrell, 1975, p. 573.) In this situation, the eventual apportionment (i.e., the estimated partial regression coefficients) is highly sensitive to small changes, as from sampling error, within the component set and between specific components and other independent variables (cf. Walker and Lev, 1953, pp. 332–34; Gordon, 1968, pp. 608–10). More precisely, among a set of variables which redundantly measure the same global variable, the bulk of their explanatory power will be assigned to the least redundant component, with greatest loss between zero-order and partial coefficient being suffered by the most redundant (i.e., by the component with the highest squared multiple correlation with the other members of the set). As it happens, occupation is the most redundant of common social class variables. *This is why it often* suffices as the sole measure of class. Income, and income-related items like owning an automobile, a private home, and a telephone, are the least redundant. In this case, therefore, a pure artifact of the application of multiple regression to a situation of correlated independent variables would produce precisely the pattern of findings reported by Franklin and Mughan. There exist a number of procedures for diagnosing problems of interdependencies among independent variables (Farrar and Glauber, 1967; Irwin, 1971). While none is unambiguous, the absence of any such examination of these data makes it difficult to accept the conclusions of Franklin and Mughan in the face of a plausible counter-interpretation arising from their uncritical use of regression procedures.

Substantively, the inclusion of several related components of social class in the same equation can lead to mistaken inferences about larger theoretical questions. For instance, the net effect in this case is to give the impression of a decline in production-side variables and a rise in consumption-side elements of social class. It may be that this is indeed happening: there is little doubt that status polarization is diminishing in Britain, and occupation may also be declining as a determinant of class-related dispositions (cf. Crewe, et al., 1977). My argument is that the latter question cannot be answered by employing regression techniques as these authors have done.

The moral of this note is relatively simple: statistical procedures often do not work well if employed in tasks for which they are inappropriate. Multiple regression is designed to calculate the causal impacts of numerous independent variables—independent not only in the sense of not being dependent, but also of being independent of each other. In practice, of course, some correlation among independents is quite tolerable, but this is a far cry from using the procedure to sort out the effects of variables which, both theoretically and empirically, are related as parts of a composite whole. On the constructive side, it should be pointed out that it is not by any means impossible to isolate with fair accuracy the separate effects of component variables (provided they are not identical) and that to do so, old-fashioned physical controls (i.e., by cross-tabulation) are preferable to statistical ones (i.e., by regression or partial correlation). Especially with few variables and many cases, elaboration analysis using cross-tabulation is a realistic possibility. Franklin and Mughan, for instance, include five or six variables and have access to some 6000 cases. A compromise, which provides a role for the more powerful regression techniques, would be covariance analysis, or one of the adaptations of the general linear model to the analysis of conditional relations (Fennessey, 1968; Lehnen and Koch, 1974; Wright, 1976). Any of these approaches to elaboration analysis would be preferable to the unstable and often idiosyncratic estimates produced by simple multiple regression applied without regard for its underlying assumptions.

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Reply

TO THE EDITOR:

In his critique of our analysis of the decline of class voting in Britain, M. Stephen Weatherford suggests that the changing pattern of coefficients we observe for the explanatory power of occupational class and other variables could be the spurious consequence not so much of the decline of occupational class as of its increasing redundancy as an indicator of social class, viewed as a more complex phenomenon. This criticism is well taken, and serves to underline our own stress on the fact that all multivariate analyses of social processes rest in part on shifting sands. In reply we wish to make one general point supported by three specific ones.

On the general point, we did not propose the use of multiple regression analysis as absolutely the best technique available for handling this type of problem (Franklin and Mughan, 1978, p. 527, n.12). We chose it because it permitted us to highlight the deficiencies of AID by producing comparable coefficients, and then to show how yet a third view of the decline of class voting could be afforded by causal modeling techniques. The use of different methods of analysis for each step in our argument would have greatly complicated our task, and, despite its deficiencies (see p. 527, nn. 12 and 14), we do feel that multiple regression analysis has generally proved itself robust enough in the face of violations of many of its basic assumptions to serve as an adequate (if not ideal) tool in most contemporary political research situations. Political researchers seldom know enough about the subject matter under investigation to be much concerned with discrepancies of one or two points in the least significant of even two significant digits. Only if greater discrepancies arise from multicollinearity would researchers find themselves with anomalies in need of methodological explanation.

This brings us to the first of our specific points, which we raise in the form of a question. When does an anomaly become a methodological artifact? For there is certainly an anomaly in our findings. Bivariate analysis of the relationship between occupational class and voting choice shows this relationship in 1970 to have declined by 40 percent of its 1966 value, from an r^2 of .091 to one of .055 (Franklin and Mughan, 1978, Table 1). However, multivariate analysis of the same relationship shows it to have declined by 55 percent, from an r^2 of .029 to one of .013 (Table 3). In both years other variables are capturing a large part of the variance which, in the bivariate situation, is explained by occupational class; but by 1970 other variables are capturing a greater proportion (76 percent) than in 1966 (68 percent). If we view all these variables as indicators of one underlying phenomenon, then it is clear that occupational class had become a more redundant indicator of this phenomenon in 1970 than in 1966. But this need not of itself constitute an artifact of multiple regression analysis. Had there been a true increase (absolute or relative) in the intercorrelations of the different indicator variables, then any other form of elaboration analysis would equally have shown an increase in redundancy. However, there is the possibility that redundancy will have biased the calculation of regression coefficients in both years under study, and that any increase in redundancy will have amplified this bias in 1970. Such a bias would indeed constitute an artifact of multiple regression analysis, but it is important to note that only a real increase in the redundancy of one of our predictor variables gives to the multiple regression analysis the opportunity to exaggerate or dampen this change. Only if a bias is great enough to make us take note of a change that we would otherwise have ignored (or to ignore a change that we would otherwise have noted) *is it likely to affect us in practice.*

This brings us to our second specific point. Weatherford suggests that a cross-tabulation analysis would be free of the artifact which he attributes to regression analysis, and when the pattern of decline in class voting is estimated by cross-tabulation, *it is indeed different*. In a six-way contingency table of Labour vote against each of the other binary variables included in our causal model (p. 531), each sub-table gives the effect on the probability of voting Labour of a change in one characteristic, given a particular combination of other characteristics. If we measure this effect by taking the difference in the proportion voting Labour between those having the characteristic and those not having it, this yields a coefficient (Somer's D) analogous to the *b* coefficient in regression. By taking a weighted average of this coefficient from each of the 16 sub-tables corresponding to different combinations of characteristics on the other four independent variables, we get a result comparable to the regression coefficient.[1] Table 1 compares these

Table 1. Regression Estimates Compared with Those Deriving from Cross-Tabulation in Assessing the Importance of Five Predictors of Voting Choice, 1966 and 1970

Independent Variable (Labour Characteristic)	1966		1970		Decline	
	Regression	*Xtab*	Regression	*Xtab*	Regression	*Xtab*
Occupational class (working)	.124	.115	.080	.075	.044	.040
Education (less)	.137	.128	.078	.077	.059	.051
Tenancy (tenant)	.184	.184	.180	.178	.004	.006
Union membership (member)	.158	.157	.146	.144	.012	.013
Telephone ownership (none)	.124	.118	.122	.120	.002	.008

Source: See Mark Franklin and Anthony Mughan (1978). "The Decline of Class Voting in Britain: Problems of Analysis and Interpretation." *American Political Science Review* 72: 523–34.

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[1] The D for each sub-table was weighted by the reciprocal of the sum of the variances for each row contributing to its calculation, and the final total divided by the sum of these weights. If P_i is the proportion voting Labour among those (N_i) having an attribute, and P_j the proportion among those (N_j) not having it, then

$$D = \frac{\sum_{i,j=1}^{k} (P_i - P_j) \dfrac{1}{(P_i(1-P_i)/N_i) + (P_j(1-P_j)/N_j)}}{\sum_{i,j=1}^{k} \dfrac{1}{(P_i(1-P_i)/N_i) + (P_j(1-P_j)/N_j)}}$$

where k is the number of sub-tables. The equation is adapted from Davis (1975) who follows Goodman coefficients for each of the variables employed in our causal model for each of 1966 and 1970. The last two columns compare the decline in the effect of each variable between 1966 and 1970, as estimated by each type of analysis.

Viewing each pair of columns from the perspective of our present level of understanding of social processes, one feels that the differences between equivalent coefficients are hardly more than trivial, averaging no more than .005 for any pair of columns, and never exceeding .009 for any pair of coefficients. However, when we look at the final pair of columns, even though the differences are no greater than elsewhere, we do see a systematic distinction between class and education on the one hand, where regression analysis shows the declining effect to be somewhat greater than does cross-tabulation analysis, and tenancy, union membership and home ownership, on the other hand, where regression analysis shows the declining effect to be somewhat less than cross-tabulation analysis. These differences would be consistent with a regression artifact which exaggerated the declining importance of two variables that were becoming relatively more redundant indicators of an underlying phenomenon while dampening the decline of three variables that were becoming relatively less redundant.[2] In our article we did draw a distinction between these two sets of variables based on the apparently more rapid decline in the importance of occupation and education. But the cross-tabulation coefficients show the same clear distinction, as they would have done even had the regression artifact been two or three times what is shown here. If these figures are at all typical of the bias that can be introduced into regression estimates by changes in multicollinearity, then it seems unlikely that we will be seriously misled in practice by this particular artifact of regression analysis. Certainly there are other everyday problems of data analysis (such as how to deal with missing data) that should concern us more.

We are left with the fact that, viewed as an indicator of an underlying class phenomenon, occupation did become more redundant in 1970, and this brings us to our last specific point. We neglected to mention this clear implication of our findings. Does it matter? We would argue that it was only peripherally relevant. Our main objective was to point out the different interpretations that would be placed upon the declining power of occupational class to structure partisanship when explanatory power was apportioned among predictor variables in three fundamentally different ways. We conclude (p. 533) that the basic choice is between using occupational class to explain or to predict voting behavior. It is this choice which leads to the selection of causal modeling or regression analysis as an appropriate technique. Both the causal model and its regression counterpart are affected by the relatively greater redundancy of occupation in 1970, but if one is taking a causal view, then this is no more than a concomitant of the increasingly indirect effects of class. On the other hand, if one views social class as an indivisible phenomenon that cannot be split into pre- and post-adult stages, then one has to conclude that the utility of occupation as an indicator has declined. Although we do favor the causal approach, we did not seek to assert that this view was the correct one. Our primary objective was simply to point out that coefficients of different magnitude derive from taking one or the other view.

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(1963) in proposing this as the maximum likelihood estimator of D in the absence of interaction. The adaptation consists of normalizing after summing rather than before, so as to permit a one-pass process on a programmable pocket calculator.

[2] It is also consistent with a regression artifact that boosted the apparent impact of occupation and education in 1966 but not in 1970. These are the two regression coefficients that depart most from their cross-tabulation counterparts. Although the redundancy of occupation was relatively less in 1966, it was in fact greater in absolute terms (by an average of 1 percent of the variance explained), and this absolute difference in multicollinearity (rather than the relative differences we concentrate on in the text) might be responsible for the artifact. If so, it is not the artifact we expected to find from a reading of Weatherford's critique of our work, and it could as easily be due to some other violation of the assumptions of regression analysis on the part of these two variables in 1966.

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Comment on Gow

(Vol. 73, June 1979, pp. 430–41)

TO THE EDITOR:

David John Gow has contributed significantly to Glendon Schubert's psychometric model for judicial decision making by discovering objective mathematical procedures that find optimal locations for scale vectors in multidimensional spaces. Schubert has noted this problem (1965, p. 73; 1974, p. 73), and Gow should be commended for solving it. An optimal location for the scale vector can now be found, but a statistical measure has yet to be devised to determine the significance of the correlation between the rank orders of a scale vector and its corresponding scalogram. Schubert does not provide such a test when he measures the probability of chance correlation by employing the significance levels of the correlation coefficient used for locating the scale analogue.

Schubert, in accordance with the psychometric model, locates the scale analogue in the space by finding a scale vector with a rank order that most closely matches the rank order of the scalogram, as measured by some correlation coefficient. Schubert then measures the probability that the scale vector represents the attitude associated with the scalogram by the significance level of the coefficient used. If the significance level exceeds a certain floor level, such as one chance in 10,000, he assumes the possibility of a chance correlation can be reasonably ruled out.

Significance levels should not be used this way. They measure the probability that two *independently derived* orderings are related by chance. If, for example, the scale vectors were located in the space without resorting to the scalogram ranks, and the orderings of the corresponding scale vectors and scalograms were then compared by a correlation coefficient, using the coefficient's significance levels would be proper. (This assumes statistical independence of the derived *i*-points and scalograms, although as Gow notes [1979, p 431], these two procedures are only "somewhat independent," because they are derived from the same data of voting behavior.)

The scale vectors, however, are not derived independently of the scalogram ranks. Indeed, the rank order of the scale vector is *designed* to correlate as closely as possible to the corresponding scalogram's rank order even before the two ranks are compared by the significance level of the correlation coefficient. The scale vector located by the model is by definition the vector that most closely resembles the ordering of the associated scalogram. Clearly, a vector designed to have the best fit will almost always have a much higher significance level than one derived independently. Thus, the significance level derived by this procedure significantly underestimates the probability of a chance correlation.

Consider the problem from another perspective. Let the points (0.1, –0.7), (0.0, –0.3), (0.3, 0.7), (–0.7, –0.4), (–0.2, 0.9), (0.6, –0.2), (–0.3, 0.3), (0.5, –0.6), and (–0.4, –0.8) represent the ideal- (or *i*-) point locations of nine justices in a two-dimensional factor plane. Any scale vector in any space can represent either one of a pair of "mirror image" rank orders, depending on the direction given to the vector. For *n* points, there are a total of *n*! different rank orders of the justices, so there are *n*!/2 distinct potential scale vectors, each representing a pair of distinct potential rank orders, as noted by Gow (in a personal communication). In this example, there are 9!/2, or 181,440 pairs of possible orderings of the justices. There are 36 different pairs of rank orders (excluding orderings with tied values) of the justices that can be represented by a scale vector in the factor plane,[1] so $\frac{36}{9!/2}$, or about 0.02 percent, of all the possible orderings are so represented. Thus, the probability that a *random* ordering of the justices can be represented by a scale vector with a *perfect* correlation is 0.02 percent, 72 times higher than the 0.0003 percent (or $\frac{1}{9!}$) indicated by a correlation coefficient significance level.

As the number of dimensions employed by the psychometric model increases, the significance levels of the correlation coefficient will underestimate the chance of a spurious correlation by an increasingly higher probability. Every possible rank order of *n* items can always be represented by a scale vector in a space of *n* − 1 dimensions, as noted by Gow (personal communication) (see Coombs, 1964, p. 156). Every possible ordering of nine justices, for example, can always be represented by a scale vector in a space of eight dimensions, regardless of the configuration of the justices' *i*-points. As the number of dimensions increases from two to *n* − 2 dimensions—the range of dimensions psychometric analysis normally uses—a successively larger percentage of orderings can be represented by scale vectors in the space. It is conceivably possible, for example, for there to be 756 different pairs of orderings for seven points that can be represented by scale vectors in a four-dimensional space. If so, using the significance level for a perfect correlation between two orderings of seven items will underestimate the probability of a chance correlation by a factor of 1,512 times (see Bennett, 1956, p. 388; Coombs, 1964, p. 272).

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[1] Scale vectors that intersect the following points each derive a different pair of orderings of the justices without any tied values: (0.0, 1.0), (0.2, 1.0), (0.38, 1.0), (0.5, 1.0), (0.57, 1.0), (1.0, 1.0), (1.0, 0.8), (1.0, 0.745), (1.0, 0.6), (1.0, 0.49), (1.0, 0.45), (1.0, 0.35), (1.0, 0.3), (1.0, 0.2), (1.0, 0.17), (1.0, 0.16), (1.0, 0.0), (1.0, –0.1), (1.0, –0.12), (1.0, –0.15), (1.0, –0.2), (1.0, –0.26), (1.0, –0.35), (1.0, –0.4), (1.0, –0.5), (1.0, –0.6), (1.0, –0.9), (1.0, –0.95), (0.8, –1.0), (0.65, –1.0), (0.5, –1.0), (0.23, –1.0), (0.22, –1.0), (0.18, –1.0), (0.16, –1.0), and (0.15, –1.0).

The probability of deriving a scale analogue that represents an ordering with a spurious correlation is further increased when the researcher accepts scale analogues with less than perfectly matched rank orders. A researcher using the psychometric model typically will find the location of a scale vector acceptable if the significance level indicates that the probability of a spurious correlation is less than, say, one chance in 10,000, such as when the Kendall's *tau* correlation coefficient for orderings of nine items is 0.90 or greater, rather than demanding the more stringent requirement of a perfect correlation, such as when Kendall's *tau* for nine items equals unity.

The significance levels for correlation coefficients are simply inapplicable for determining the probability of chance correlations between scalogram and scale vector orderings. They underestimate the chance probability for rank order correlations by a large margin whenever the psychometric model uses at least two dimensions, and they underestimate the chance probability by a significantly larger margin as more dimensions are used.

Gow has suggested (in a personal communication) two alternative methods to measure the degree of internal validity actually indicated by the correlation value attained by fitting the scale analogue in space. First, a procedure similar to a Monte Carlo simulation might be employed to estimate the probability of a chance correlation adjusted to the number of dimensions being used. A series of trials, each randomly selecting (1) a configuration of *i*-point locations for *n* items in a *k*-dimensional space and (2) an ordering for *n* items, could be generated by a computer. For each trial, an optimal location for the ordering's scale analogue, in relation to the *i*-point configuration, and the value of the correlation coefficient associated with this optimal scale vector could then be determined using the computer procedure Gow developed in his 1979 article. If the data run were large enough, a table of accurate estimated probabilities could be determined for each possible value of the correlation coefficient for orderings of *n* points in a *k*-dimensional space. Second, a correlation coefficient that is adjusted to account for the number of dimensions employed by the model could be used, such as the adjusted multiple correlation coefficient ($\bar{R}$) reported by Theil (1971, p. 178), which is available through SPSS (Nie et al., 1975, p. 358).

In his article, Gow develops mathematical procedures to determine objectively a scale vector that best fits the data. A statistical measure should now be discovered to determine whether this best fit is actually a good fit.

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Reply

TO THE EDITOR:

I saw no need to comment on the principal article by David John Gow, who was for six years a doctoral candidate of mine and who, *inter alia,* had worked under my direction, at various times during that longer period, on the problem of the subject article. That question had been addressed earlier by Fred Kort of the University of Connecticut, and discussed in correspondence between him and myself on and off over the past 20 years. Now we have been presented with Gow's claim to have (at long last) solved this puzzle by suggesting an appropriate set of mathematical solutions as a substitute for the ad hoc procedures which, as I had deliberately and frequently announced, were ones upon which I had relied in earlier research pending the discovery of a better method. If, as time will tell, Gow has now found such a better technique, then I am delighted and congratulate him, and not least on his persistence. The Voorhis comment, although nominally on Gow is in fact on my own work, and it raises very different questions that have nothing to do with those with which Gow was concerned.

Coombs' *Theory of Data* is cited and discussed at length in *The Judicial Mind* and *The Judicial Mind Revisited* as a major premise of the psychometric model proposed by me therein. The crux lies in how "data" are to be defined, and Voorhis appears oblivious to Coombs' critical distinction between "raw data" and "data." A set of published *United States Reports* contains raw data. Observations of those *Reports* to construct cumulative scales based on a particular theory and method of classifying the content yields one form of data. A separate and independent observation of those *Reports* to make very different observations, based on a different theory and method of factor analysis, produces a separate form of data. As Coombs points out, and as I point out in *The Judicial Mind Revisited,* at a higher level of integration in multidimensional theory, scales are *conceptually* components of factorial spaces; and therefore the hypothesis that scales ought to be highly correlated with factorial configurations is justified. But the two data sets employed to make that test empirically remain entirely independent statistically; if the theory is to be falsified, one way to do it is to show that voting relationships among Supreme Court justices are random—then there can be no clusters and scales, *except* by chance. If both clusters and scales are manifest, and they constitute equivalent modes of representing—the one multilinearly and the other unilinearly—the same set of response differentials, then the hypothesis is not disconfirmed. A test of statistical significance for non-chance explanation of the observed isomorphism is appropriate because the two data sets *are* independent.

My claim, in both *The Judicial Mind* and *The Judicial Mind Revisited,* is precisely that the data I analyze there support the substantive theory that the votes of Supreme Court justices are *not* independent, but that instead such votes are highly *dependent* and *interdependent.* It should be clear, however, that that substantive theory has no bearing upon the point raised by Voorhis, which is strictly one of methodology and concerns only the independence of data employed in the test of whatever substantive theory one might employ. But his confusion is epistemological and (as he does not seem to recognize) involves a question at the level of the philosophy of science: how and why observations of empirical events ought to be conceptualized for analytical purposes.

Voorhis' attention does not seem previously to have been directed to an earlier article of mine (1962) that is most readily appreciated in the context of the first and third articles preceding it in a subsequent reprinting (1964), and which I do discuss in both of my books (1965, 1974) at issue here. There I analyzed scale data collected by Pritchett and factorial data by Thurstone—entirely independently, at another time and for quite a different purpose, and in each instance collected long before and therefore necessarily in ignorance of my subsequently formulated theory, methods, and psychometric model. It was only *after* I had made that and other similar tests that I began to feel justified in taking the model more seriously myself; and it is difficult to imagine a more rigid example of independence in data—under any epistemology—than this one.

If Voorhis were to appraise the scoring of (say) a panel of judges of an Olympic gymnastics performance (say, free exercise), would he argue that a test of the independence of ratings (vis-à-vis a chance mechanism for evaluation) is rendered inappropriate by the circumstance that all of the Olympic judges observed the same tumblesault? Or that a test of the independence of votes of Supreme Court justices is inappropriate because all have read the same briefs and heard the same oral argument?

GLENDON SCHUBERT

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On Book Reviews

Comment on Ranney's Review (Vol. 73, December 1979, pp. 1145–47)

To the Editor:

Austin Ranney's review of my book *Decision for the Democrats: Reforming the Party Structure* (1978) requires comment. Austin and I disagree on the need for and effects of the movement for party reform. In fact, we may well represent the opposite ends of the spectrum in our views.

A few of Austin's specific points need attention. My accounts and explanations of the evolution of the reform movement are based in large part on my participation in it. I had the good fortune to spend 18 months in Washington working on reform at one stretch in addition to an average of three months for each of the succeeding years during the period 1969–1974. I was able to serve on the staff of two of the reform commissions and as a consultant to four of them. I had full access to the staff, to the reform commissions and to *all* materials. My belief is that I had more extensive information concerning and more knowledge of what was occurring and why than many of those prominent in the movement. As a consequence, what I take to be Austin's central point concerning data is misdirected. My major problem in writing the book was to find outside documentation that I could use to provide some type of written record for what I was developing and I went to lengths to provide the reader with such sources. Whatever limits the book might have, lack of access to data and exposure to the events and personalities of the day and their views on and role in reform cannot be one of them.

In another vein, I included commentaries by other academics or professional advocates only to the extent they spoke to my concern with depicting the causes, chronology and implications of the reform movement. I felt that the book was not the place to argue the validity of the points of view of different observers or the strengths or shortcomings of their work. This concern was not core to my endeavor.

In these regards, I have dealt elsewhere with the arguments made in Jeane Kirkpatrick's book on the 1972 national conventions. Austin is aware of this. I feel that Jeane's arguments miss the point. The presidential nominations are now decided in the preconvention phase by the grassroots activists. The national conventions, as a result, reflect the characteristics of the winning candidate's coalition.

Byron Schafer's dissertation is not available either from Berkeley or University Microfilms and, in fact, has been restricted from circulation by the author. While it may add additional information, I believe it unlikely that it would alter my perception of the events I witnessed.

The book makes it clear that the staff had a major role in structuring commission deliberations (one reason why Austin and other commissioners were put in the role of reacting to alternatives they often did not fully comprehend) and that the staff, along with key individuals prominent in the reform process, account for much of the success in enacting the reforms. Larry O'Brien played a role in this regard. Initially cautious about making any commitments to the reformers, his support at important junctures contributed to the reform success. These points are made in the book.

Within *Decision for the Democrats,* I judge the reforms by the objectives they sought to accomplish. This is made painstakingly clear, as are my own values in approaching the topic.

The reform movement's biggest offense was in succeeding. In doing so, it has challenged not only the old ways, but the conventional wisdom of the political scientist, much of which is now outdated. The different strains of thought are evident in the review and account for the opposing views held by Austin Ranney and myself. Once these are clear, the book can stand or fall on its own merits.

William Crotty

Northwestern University

EDITORIAL NOTE

The article in this issue by William H. Riker ("Implications from the Disequilibrium of Majority Rule for the Study of Institutions") is the first of occasional "Research Frontier" pieces to be published by the APSR. Such articles go beyond the more traditional review essay to explore paths of future research. Typically we will invite comments on such articles. In this case Peter Ordeshook and Douglas Rae were asked to prepare brief responses to Riker's statement. All such work is refereed.

The following articles have tentatively been scheduled to appear in the September, 1980, issue:

Thomas E. Mann, American Enterprise Institute, and Raymond E. Wolfinger, University of California, Berkeley, "Candidates and Parties in Congressional Elections"

Alan I. Abramowitz, College of William and Mary in Virginia, "A Comparison of Voting for U.S. Senate and Representative in 1978"

Barbara Hinckley, University of Wisconsin, "Toward an Understanding of Congressional Elections"

John Aldrich, Michigan State University, "A Dynamic Model of Presidential Nomination Campaigns"

David O. Sears, Richard R. Lau, Tom R. Tyler, and Harris M. Allen, Jr., University of California, Los Angeles, "Self-Interest vs. Symbolic Politics in Policy Attitudes and 1976 Presidential Voting"

Eric R. A. N. Smith, University of California, Berkeley, "The Levels of Conceptualization: False Measures of Ideological Sophistication"

John G. Peters and Susan Welch, University of Nebraska, "The Electoral Effects of Corruption Charges"

Bernard Yack, Harvard University, "The Rationality of Hegel's Concept of Monarchy"

James T Knauer, Lock Haven State College, "Motive and Goal in Hannah Arendt's Concept of Political Action"

Richard F. Bensel, Texas A&M University, "Creating the Statutory State: The Implications of a Rule of Law Standard in American Politics"

Michael S. Lewis-Beck and John R. Alford, University of Iowa, "Can Government Regulate Safety? The Coal Mine Example"

John F. Hoadley, Duke University, "The Emergence of Political Parties in Congress, 1789–1803"

Errata

Two errors appear in James Kuklinski's "Political Participation and Government Responsiveness" (December, 1979). On p. 1093, lines 12–13 of the first column should read: "8 percent in 1971 and a meager 3 percent in 1972." In the second column, 13 lines from the end, the words "the cases increase" should be deleted.

BOOK REVIEWS

Political Theory and Methodology

The State as a Firm: Economic Forces in Political Development. By Richard D. Auster and Morris Silver. (Boston: Martinus Nijhoff, 1979. Pp. x + 178. $14.95.)

The interesting thing about economists is not their immodesty nor their unlimited confidence in their own theories. Protected by the ghost of Adam Smith, argument by analogy, and the immortal words, "We assume that. . . ," they are willing to tackle anything. No, what is interesting about economists is that, despite themselves, they often generate some interesting ideas. The book under review here is a good example of this phenomenon. If you take the authors seriously and consider their work a theory of the state or as a contribution to a theory of the state, this is neither an interesting nor important book, especially if you believe there is something called politics which has to do with states and governments. There is little about politics in this book. The authors take as given an analogy between firms and states without examination or any consideration of when it does and does not hold. But on the other hand, if treated discretely, quite a number of the book's interesting ideas and hypotheses are relevant to theories of the state as well as to theories of how state governments operate.

Particularly interesting is Auster's and Silver's analysis of "Democracy, The Corporate State." In their view, the purpose of democracy is, in principle, to diffuse ownership of the state so as to "redistribute the profits accruing to the monopoly power of the state among the citizenry as a whole" (p. 73). However, as in practice the stockholders of a corporation have little to say about the running of the corporation, "representative democracy attempts but does not in fact succeed in changing the ownership-control of states into one of complete equality. Public workers, including politicians become the new owners" (p. 74). But while public workers are the owners, their ownership and property rights are very diffused and as a result there is little relationship between their level of profit and state performance. This leads to (1) a faster development of "institutional entropy" in democracies than in monarchies or oligarchies (where there is a "residual income recipient"), and (2) a "backward-bending" supply curve so that increasing demands for state services "may actually reduce the level" produced (p. 74). After examining these consequences, Auster and Silver conclude that it is irrational to prefer democracy on grounds of equity or efficiency. But people seem to prefer democracy; and as they cannot be irrational, they must prefer democracy as a "consumer good"—as an end in itself, regardless of its other characteristics. As they are arguing by analogy between firms and states, are we also to conclude that people prefer firms as consumer goods, too?

This book is likely to make a big splash among the relatively closed group of public choice economists; undoubtedly many references will be made to it in the future. If you are in this category, you will probably want to read it. Otherwise, however, you can ignore it at no cost to yourself.

GERALD STROM

University of Illinois, Chicago Circle

For Capital Punishment: Crime and the Morality of the Death Penalty. By Walter Berns. (New York: Basic Books, 1979. Pp. x + 214. $10.95.)

Walter Berns fairly compiles pros and cons in this elegantly written essay. He concludes that the death penalty is needed (for treason, some rapes and some murders [p. 183]) as retribution, to enhance the "dignity" of the criminal law (p. 173), and to express the anger (the "passion" that connects punishment to justice) of the community against criminals (pp. 147, 152–54). I, too, am for capital punishment. But I find Berns' compilation careless and too often confused. I shall proceed synecdochically, and from minor to major objections.

(1) If execution is threatened for rape, rapists are invited to murder their victim-witnesses without fear of additional punishment.

(2) Berns writes, "Except in the case of organic disease of the brain, there is no scientific basis for the use of the term mental illness" (p. 69), but also: "[some] men . . . because they are insane are not responsible for their acts" (p. 153). Either insanity is coextensive with "organic disease of the brain," to Berns, or he is inconsistent. I hope for the latter; else the M'Naghten rule would have to go, and insane

murderers without organic brain disease such as M'Naghten could be executed.

(3) Berns pads his account with unrelated matter, and misconstrues it. Thus Shakespeare's Macbeth is discussed (pp. 164–70) as though the subject were punishment rather than murder and guilt. And there are irritating *obiter scripta:* "Only a ruler or a contender for rule can act with the freedom and on a scale that allows virtuousness and viciousness to be fully displayed" (p. 169). Very classical, but patently false in literature and life.

(4) Berns' references are erudite but erratic. His deontological argument is simplified Kant–yet that philosopher is not given the credit. His antagonist is Bentham–but he focuses on Beccaria, a comparative lightweight.

(5) For Berns the death penalty need only be deserved as retribution; it need not have utilitarian benefits. But he often confuses matters by presenting actually utilitarian arguments as though arguments for a "just deserts" theory. Thus, if the death penalty is needed, as Berns asserts throughout, to express communal anger, to denounce the crime and blame the criminal, to "reward" and make the law-abiding "happy" (p. 147), to teach respect for law and uphold its "dignity," it is a means to these crime-reducing (utilitarian) purposes. However, if the death penalty is a moral necessity, a categorical requirement of justice, as Berns believes, the mixed bag of unrecognized utilitarian arguments which he offers is irrelevant to its justification. Thus a crime may well justify anger springing from injured moral feeling. But it is hard to see how this anger in turn makes any punishment just or moral. Anger–whatever its basis–explains or motivates, but does not justify or constitute an argument for legal action, unless it be on the utilitarian ground that the anger must be legally discharged to avoid illegal action.

(6) "Deterrence," Berns claims, "is incompatible with the principle of just deserts" (p. 137). But the latter wants to "pay back" for a past act, whereas deterrence wants to prevent future acts. The principles deal with different subjects and therefore are compatible, actually even complementary. They may lead to different punishments, but Berns himself admits that they need not. "The modern purpose," Berns adds (p. 138), "is not to teach men that it is immoral to commit a crime . . . but that it is contrary to their interests. . . ," as though these purposes were anything but complementary. The immorality of crime does not suffice to deter wrongdoing, wherefore crime is made "contrary to [the criminal's] interests" by means of punishment.

(7) Following Kant, Berns regards the death penalty not as a (necessary) evil which must be offset by the good effects it produces, but as a positive good *per se* which need not be. Murderers are to be executed because they deserve to be. I cannot see how desert here is anything more than a feeling disguised as an argument. Perhaps an argument is not needed to justify acting on this feeling. Perhaps there is no argument. At any rate, I share the feeling but balk at the disguise.

Criminologists distinguish "instrumental" from "expressive crimes." Berns seems to suggest *expressive punishment* (as I would call it). I do not object–but can the expressive function of punishment be a better argument for punishment than the expressive function of a crime is for crime?

Berns could not be expected to solve the basic problems of deontological retributionism which have puzzled political philosophers for centuries. But he could have met them. Rhetoric–conservative or liberal–is of no help.

ERNEST VAN DEN HAAG

New School for Social Research
New York Law School

Paradoxes of Education in a Republic. By Eva T. H. Brann. (Chicago: University of Chicago Press, 1979. Pp. vi + 172. $12.95.)

In this slight but powerful volume, Eva Brann has made an important contribution to the recently revived and increasingly vigorous national discussion of liberal education. Concerned especially with the college years, she focuses her inquiry on several paradoxes of American education, paradoxes, she believes, characteristic of education in a republic and especially "amenable to reflection and resolution" (p. 1) when they are seen originating in the founding years of the American Republic. Her sources for describing the paradoxes and for aiding her inquiry into them range from recent national studies on American education to classic works by authors such as Plato and Quintilian. She obviously treasures Alexis de Tocqueville's study of American democracy while pointedly resisting any servility to his view of the limitations of democracy. She has, one might say, a Jeffersonian hopefulness about the possibilities for the American republic, though it is in Jefferson in particular that she finds paradigmatic and influential forms of the intellectual tendencies that must be overcome if American education and the American Republic are to reach their human potential. No student of Jefferson, whether hostile or friendly through previous study, should miss the encounter with Jefferson in this book.

The "grave difficulties" or paradoxes that Brann discerns are gathered under the topics of *utility*, *tradition* and *rationality*. The problematic character of a pervasive utilitarianism, an equally pervasive repudiation of tradition and an appreciation for reason exclusively in instrumental and formal terms is located in American thinking about education, but it is ultimately traced to the Enlightenment whose thinkers formed the American Founders and thereby the spirit as well as the letter of the American Republic. To see the paradoxes in the American Enlightenment is to see them not only more clearly but also in the thought of those more aware and usually even more appreciative of what was being repudiated in the name of modernity than those who follow. To see the paradoxes there is to see their roots and to see better the possible resolutions. That Brann can speak of these paradoxes as "exhilarating and appalling incitements to inquiry" and as "heroic flaws" is an indication of her own dual appreciation (p. 148) for the American experiment and a pre-Enlightenment tradition. She seeks to join the American experiment to this tradition rooted in Plato and Homer; she finds the former in need of the latter even as she recognizes that shapers of the former like Jefferson worked, sometimes despite themselves, to fracture the links with that tradition in America.

What resolutions Brann offers add up to a sketch of undergraduate liberal education that is a convincing alternative to what so commonly prevails and that clearly represents the aspirations of St. John's College (Annapolis and Santa Fe) at which she is a dedicated tutor and philosophical guide. Regarding *utility*, it is argued that liberal education is not impractical but pre-practical, that useful learning and liberal learning are complementary, specifically insofar as liberal learning provides the inquiry into ends that useful learning stands in need of. Furthermore, "solid and deep liberal learning" rather than "high-powered institutional education" is the more reliable preparation for republican leadership and citizenship (p. 58).

Some sense of what that liberal learning consists in can be garnered indirectly as well as directly from other observations made in the course of Brann's inquiry. Such learning is not "proudly gratuitous" (p. 31) pure humanistic scholarship, nor the ornamental learning that has so often passed for a gentleman's education. And she warns against those curricular approaches that emphasize current social and personal problems, arguing that students will deal with such difficulties better, "indeed will formulate them, in a much more trustworthy way for having been inducted into the goods of their own civilization and for having taken the time to acquire some cultivation" in "the best that has been thought and said" in that civilization (p. 29).

Education in this liberal tradition is essentially learning to read; it is reading in the profound sense of repossessing by recovering meaning and reappropriating reflection. This is a reading, above all, of books that are great exemplars of original and significant inquiry. It is the Western tradition of such inquiry that makes up Brann's curriculum, and the books in which it is found are sharply contrasted to the textbooks that abound in response to the Enlightenment's emphasis on the diffusion of knowledge. It is the *tradition* of inquiry that Brann seeks to restore to American college education; she seeks it not in curatorial spirit of preserving the past, but in what could be called an American way of reappropriating the past for the sake of the future. The inquiry she finds in the tradition is the corrective to the instrumental *rationality* that unduly dominates America. Inquiry is the receptive intellect's mode of questioning. It points beyond formalism and methodology and is open to substance, and yet it is unqualifiedly free and therefore consistent with the way of the republic.

There is at times a touch of opinionated rashness in this book, such as in the blanket condemnation of educational experiments, the caricature of the American emphasis on education for the future and the condemnation of humanists like Erasmus as "flaccid edifiers." Especially in such moments Brann's own discourse seems impressionistic (lacking the "taut grip of real reflection" (p. 6) that she misses in the humanists and their modern heirs) and likely to draw agreement and acclaim only from those already convinced. Defenses of liberal learning require an unclouded clarity and compelling persuasiveness in these times. The flaws in this book, however, hardly deserve a passing note in the light of the debt due Brann for her achievement here in reappropriating the tradition of liberal inquiry and making a fresh statement on its applicability to the American Republic. Her statement can and should contribute to renewing the most significant reform movement in twentieth-century American higher education, the movement for a coherent undergraduate curriculum centered on the best books of the Western tradition.

WALTER NICGORSKI

University of Notre Dame

Why Nations Act: Theoretical Perspectives for Comparative Foreign Policy Studies. Edited by Maurice A. East, Stephen A. Salmore, and Charles F. Hermann. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 256. $15.00, cloth; $7.50, paper.)

This book is a product of the CREON Project (Comparative Research on the Events of Nations), an interdisciplinary, multi-university research project on the comparative studies of foreign policy behavior. It has four basic orientations. First, foreign policy is viewed as the product of individual foreign policy decision makers operating in an organizational context. Second, foreign policy behavior is complex, and can be studied best by developing multivariate causal models susceptible to empirical testing. Third, causal mechanisms need to be developed explicitly. Fourth, the study of comparative foreign policy needs to be spatially and temporally general.

This book is theoretical, not empirical, and it provides the foundations for building theoretical models of comparative foreign policy behavior. Seven different theoretical perspectives for explaining foreign policy behavior are presented. These perspectives are: personal characteristics of decision makers, decision structures and processes, characteristics of the political regime, national attributes, properties of the international system, prior foreign policy behavior, and situational influences. The discussions of these perspectives follow a single format. They review the pertinent literatures, and relate their perspectives to previous studies. This is followed by theoretical discussions of the underlying assumptions for the perspectives, and presentations of hypotheses derived from these perspectives. The dependent variable, foreign policy behavior, is defined as purposive actions directed at some external entity (entities) and initiated by authoritative governmental decision makers or their representatives. Twenty-two types of behavior properties are developed to show how foreign policy behavior can be measured. The last article, written jointly by four of the contributors, attempts to build a set of multivariate, nonrecursive causal models of foreign policy behavior, using the seven theoretical perspectives.

This book is useful to both scholars and students of foreign policy behavior. It provides a summary and critical analysis of the comparative foreign policy literature. The standard format used by the authors provides easy comparisons and cross-references. The authors are careful in defining their theoretical concepts, and in limiting the explanatory boundaries of their perspectives. Many hypotheses presented in this book can also be applied to other areas of policy studies.

Despite its overall excellence, the book has some theoretical and methodological errors relating to statistical identification problems and incorrect causal inferences. These errors are important because causal modeling is the book's theory construction technique. If an equation of a nonrecursive causal model is under-identified (more unknown parameters than known pieces of information), then *no* unique estimations of the parameters can be obtained. It can be demonstrated mathematically that equations for two of the endogenous variables in the first causal model developed jointly by four of the contributors (p. 198) are under-identified. From a mathematical modeling point of view, this causal model needs to be revised before unique estimations of the parameters can be obtained.

The book also contains some incorrect causal inferences. For example, the authors state that they do not rank-order the various independent variables (perspectives) in terms of explanatory power. Rank-ordering in this instance is partly an empirical process, but it can also be *deduced* when formal causal models are presented, as they are in this book. Thus, from Model 2 (p. 205) we can deductively conclude that decision structures are more important than regime types in explaining foreign policy behavior. According to that model, "regime types" can only cause the behavior of foreign policy decision making through decision structures, the intervening variable. Therefore, mathematically, unless the correlation between the variables "regime types" and "decision structures" is perfect, the direct path coefficient will *always* be larger than the indirect path coefficients (absolute values), given the model's theoretical specification.

There are additional minor technical errors, of which I will mention one. The discussions about the theoretical perspectives often rely on correlational evidence; correlations are weak arguments for theoretical positions because a perfect correlation can be the product of spurious causes, and a zero correlation can be due to random and/or non-random measurement errors. Even with these theoretical and methodological weaknesses, however, this book is an excellent collection of articles systematically presented.

WILLIAM PANG-YU TING

University of Michigan

The Politics of Representation: Continuities in Theory and Research. Edited by Heinz Eulau and John C. Wahlke. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 312. $17.50, cloth; $7.95, paper.)

This volume is a collection of divergent works authored and coauthored by Heinz Eulau and John C. Wahlke. The reprinted works, all but two published elsewhere, were originally written over a two-decade span, the earliest a 1958 Wahlke memo to the Committee on Political Behavior, "The Problem of a Legislative Model," suggesting "role" as a useful organizing concept for legislative research.

The "continuities" of the subtitle are the focus of this work. It portrays two scholars grappling with an unresolved conceptual problem, with multiple coauthors, research sites, theories, and methods along the way. Beginning with the shared role conceptions of "The Role of the Representative: Some Empirical Observations on the Theory of Edmund Burke," the two authors go in different directions in later work, Wahlke turning from the demand to the support side of representation, Eulau (showing the influence of Hanna F. Pitkin) moving toward "emergent" responsiveness.

The new material in this volume, the introductory notes, is curiously inconsistent with the authors' intellectual histories. Principally responsible for introducing "representational role" in empirical legislative studies, neither author continued to pursue it in later work. It would seem that it failed to be useful and so was set aside for more promising paths. That story, in line with the continuity theme, would make interesting history of science. But the story is not told, because representational role is steadfastly defended, most particularly against those who attempted to employ it. Those who used role analysis to make predictions "vulgarized" it, the authors tell us. It is a curious conceptual scheme that can be understood only by its authors—who choose not to employ it.

A reading of this volume does not lead to optimism about the prospects for an empirical theory of representation. Twenty years after an enthusiastic beginning, Wahlke, Eulau, and the profession are still beginning. Eulau and Karps in "The Puzzle of Representation" (p. 55) quote Eulau, who said it well: "We have representative institutions, but like the Greeks we do not know what they are about."

JAMES A. STIMSON

Florida State University

The Anarchist Way to Socialism: Elisee Reclus and Nineteenth-Century European Anarchism. By Marie Fleming. (Totowa, N.J.: Rowman and Littlefield, 1979. Pp. 299. $26.50.)

Marie Fleming has written a long-needed biography of an important figure in nineteenth-century anarchism, Elisee Reclus (1830–1905). Reclus is well known as a prominent geographer who helped to revolutionize the study of geography by introducing sociological factors into his research. While his contributions to geography have long been recognized, his role in the development of anarchist political thought has been relatively undocumented. On the whole, Fleming has written a work which fills in that omission in our understanding of the development of anarchist political theory.

Reclus was a leading figure in the European anarchist movement for 30 years following the Paris Commune (during which he was arrested); "the Pope of the sect," one contemporary called him. Reclus has been ignored for a long time, at least in part, because he did not associate himself organizationally with the anarchist movement to the same degree as many of his anarchist contemporaries.

It was during his exile in Switzerland that he made his greatest contributions to the anarchist movement through a long-time association with a fellow geographer, the exiled Russian anarchist Pierre Kropotkin. Reclus provided much of the financial backing for Kropotkin's projects and served as editor of the anarchist journal *Le Revolte* during Kropotkin's forced absence.

Beginning from an assumption that the anarchist movement was one wing of a broader socialist movement, Fleming credits Reclus with being an important part of the crystallization of a systematic and scientific anarchist position during the 1870s and 1880s. The author makes the case that scholars have generally misunderstood anarchism by placing its label upon "any anti-statist idea." In Fleming's view, the result of such labeling has been to group together political thinkers of vastly different perspectives. She particularly questions the identification with anarchism of egoists such as Max Stirner and Benjamin Tucker.

Fleming's attempts to define the anarchist movement meet with mixed success. The separation of the egoists from the anarchist movement is quite valid. Their perspective has little in common with the goals of a stateless, communal society shared by the anarchists and Marxists and their view of human nature is sharply at odds with the cooperative tendencies stressed by all sorts of socialists.

The author also argues that the anarchists and the Marxists were both part of a broader revolutionary socialist movement which was in total agreement on both the evils of capitalism and the potential benefits of a communal, stateless, classless society, while sharply disagreeing on the tactics of moving from capitalist to socialist society. Fleming's view is quite popular in the history of political thought, but it should be stated with caution. While both Marxists and anarchists agree on the necessity of eliminating capitalism, the Marxists saw the economic system of capitalism at the root of the problem, while the anarchists focused on the capitalist state and ideology. While both factions agreed that society should eventually become classless and stateless, the anarchist vision of the future society was generally more decentralized and less technological than the Marxist view.

One must take strong issue with the author's statement that the formation of a distinct anarchist perspective can be traced to Switzerland in the late 1870s. Fleming states that when Michael Bakunin died in 1876 there did not exist a clearly anarchist position. Specifically, she downplays the significance of the Marx-Bakunin debate in the First International. Rather she sees the growing disillusionment of anarchist thinkers with the parliamentary socialists as the motor force behind the development of anarchism as a separate stream of thought.

The Marx-Bakunin debate did bring out the most basic and lasting difference between the Marxists and the anarchists—the disavowal by the anarchists of any political activity within the existing state. While Bakunin did embrace some aspects of Marxism, particularly its political economy, his break from Marx was almost complete and set the anarchist movement on a clear course away from Marxism.

However, it is not necessary to downplay the contributions of Bakunin, as Fleming does, to demonstrate the importance of Reclus to anarchist political thought. Reclus helped to develop the theory of anarchist communism which argues that the distribution of goods in a post-capitalist society ought to be entirely on the basis of need rather than the emphasis on work contained in Bakunin's collectivism.

Reclus must also be credited with popularizing the concept of "propaganda of the deed" which became the strategic guide of anarchism in the 1880s and 1890s. Though associated with violent acts of assassination and bombing, the concept was much broader and as interpreted by Reclus involved the notion that actions, not just propaganda, were needed to make a social revolution.

Fleming also credits Reclus with bringing a scientific basis to anarchist thought. While plausible given Reclus' study of geography and its application to the study of the evolution of human society, Reclus' political writings demonstrate that he was not a materialist but an idealist. Fleming quotes Reclus as saying, "When an idea grows revolution must come, it is impossible to stop it, it comes" (p. 226). He argues that the battle for anarchism is an ideological one, concentrating its fire on the prejudices of religion and theories of racial superiority.

In spite of some questionable methodological judgments, Fleming has contributed to our understanding of nineteenth-century anarchist theory by exploring the life of a figure largely unknown to historians of political thought.

GARY PREVOST

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Recent Conservative Political Thought: American Perspectives. By Russell G. Fryer. (Washington, D.C.: University Press of America, 1979. Pp. iv + 429. $10.00, paper.)

Socialist thought may be classified as conservative? And so may most welfare state philosophy? Well, perhaps; but any author who starts from the moderate left to understand American conservative thought, and who attempts to look at everyone who could be considered conservative (p. iii), will run into difficulties. Russell Fryer does justify his comprehensive view of conservatism, but ultimately the central concept, "conservative," becomes too protean.

Fryer presents a continuum of conservatism which ranges from left to right as follows: conservative liberalism (Sidney Hook, the socialist, and Reinhold Niebuhr, the welfare statist, fall in this category), liberal conservatism, pragmatic conservatism, philosophical conservatism, ideological conservatism, ultra conservatism, and libertarian conservatism (p. 309).

One would think that when a range classification arrives at "ultra," it could go no further. Indeed, the placement of the libertarian conservatives (e.g., F. A. Hayek and Frank Meyer) to the extreme right betrays the problems with the whole book. They are on the far right because "moderates" like Fryer view these classical liberals as the least practical. For classical liberalism dismisses socialism and considers it irrational. Even the ultra right is relevant enough to be oriented around the idea that socialism is the modern way, even if it is diabolical.

The moderate, modern "liberal" has tried classical liberalism, or thinks he has, and has found it wanting. Therefore, the only real alternative is socialism, naturally in a moderate form. Liberalism must be accommodated to socialism without, of course, becoming illiberal. Classical liberalism is an embarrassment to this moderate liberalism which dominates the university and intellectual worlds. Classical liberal ideals are impeccable—after all, the very name was adopted (or coopted) by the moderns. Libertarian conservatism clearly is based upon liberal values; but they are "classical," i.e., now impractical. So ignore them, or at least place them so far to the right that no one will seriously inquire over that far.

A good case can be made that modern American conservative thought revolves around the problem of conserving classical liberalism. As Fryer says: "Ironically, the libertarian conservative is close to being a classical liberal of the Lockean-Jeffersonian-Adam Smith pedigree" (p. 125). But how embarrassing to have the father of liberalism, John Locke, a conservative. So file this embarrassment away at the extreme right.

I would argue that modern American conservative thought is an attempt to fuse the classical liberalism of limited government and free markets, with the traditional conservatism of strong social (not state) networks, organizations, and obligations. This fusion is American, so it must conserve American cultural values of both liberty and social order. Fryer continually says this fusion is impossible (at least for the libertarians, p. 87) but does not say why. Of Meyer's attempt to do so, he merely concludes "that there is little evidence that his 'fusionist' efforts had more than paper-thin success, since the schism continues to hamper efforts towards a unified conservative movement" (p. 124).

Certainly, conservatism is not fully unified. Yet, even Marxists disagree on occasion. But I suggest that a reading of statements of principles of the major conservative organizations would show the dominance of this fusion. Indeed, a perusal of the platforms of the Republican party since 1964 would do so too. Frank Meyer actually wrote the statement of principles of the American Conservative Union, still the largest conservative organization.

Nonlibertarian conservatism in America is marginal in terms of ideals, adherents, and organizations, or even in having a political claim to the title of American conservatism. Do Hook, Niebuhr, Daniel Bell, George Kennan, Walter Lippmann, and Hans Morgenthau belong at all among American conservatives in any politically meaningful sense? And do Nathan Glazer, Irving Kristol, Patrick Moynihan, Hannah Arendt, George Santayana, Leo Strauss, and Peter Viereck really fit? Or are these not more European-type conservatives?

Take Viereck. He is correctly shown by Fryer to have praised the New Deal as a conservative political movement. His ideal post-World War II politician was Adlai Stevenson (p. 196). For these statements, Viereck would be a pariah at any Republican platform hearing, much less at an American Conservative Union forum. And Viereck would return the compliment. Or Moynihan: he had an ACU conservatism rating of 17 percent for the first part of 1979.

To be fair, Fryer is aware of this (p. 7). In fact, in his summaries, the libertarians *are* the focus, again, for their "unrealistic" views (p. 290). Left-conservatism, like that of Viereck and *The Public Interest* group, just has "more to offer" (p. 199). In other words, his moderate leftism shows. Yet, it is not a crippling bias, such as exists for many other studies of conservative thought. But the inevitable result of this perspective is more confusion on an already confused subject. For example, Fryer says he is not a conservative; but certainly he could fit into one of the "conservative" categories toward the left end of the continuum.

There is much to recommend the book, nonetheless. The summaries of the different philosophers are competent. Except for "ideological conservatism" (William Buckley, M. Stanton Evans, and Russell Kirk are used for this, although they could be considered fusionists also), and libertarian conservatism, the treatment is even excellent. The author skillfully weaves much disparate thought into a comprehensible whole.

If you are interested in good sketches of the thoughts of almost every significant moderate and conservative American philosopher, and a good many foreign ones also (which betrays the fact that this is not really a book on American conservative thought), this book is worth your time. If you want to understand American conservatism, you will need to look further.

DONALD J. DEVINE

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Group Theories of Politics. By G. David Garson. (Beverly Hills, Calif.: Sage, 1978. Pp. 216. $14.00, cloth; $6.95, paper.)

David Garson has written a very good book. It's an outstanding example of a carefully organized bibliographic essay: vast amounts of seemingly disparate literature are placed within an understandable framework. The book is organized largely historically, which may account somewhat for its clarity of organization. The literature consists of all articles and book reviews "pertaining to the group theory of politics" appearing in the *American Political Science Review* since its inception (1906). Additionally, the works of "twenty frequently cited group theorists" were included.

This formidable task was undertaken, astonishingly, by someone who believes (or at least concluded by the end of his examination) that group theory is conceptually flawed and empirically unsupported. It is not astonishing that Garson believes group theory to be imperfect, rather that he had the perseverance to analyze so much material in order to build a very strong case against a theory he finds personally unacceptable. It is refreshing to read a book about a given theory which is not a defense. Although Garson does conclude with a modest endorsement of the general notions of political economy, the strength of the book is the exhaustive, convincing argument against a theory once touted as a general theory of politics. It is even more interesting to note that many of those normally regarded as group theorists are regarded by Garson as contributors to its demise.

Garson begins at the beginning. To most of those who are conversant with the intellectual history of the theory, the beginning is Arthur Bentley. Garson argues that not only was Bentley not what we thought he was, he was "harmful to political science" (p. 30). Garson argues that the harm was due to the emergence of group theory as the dominant mode of writing about informal political processes. With its strong emphasis upon "normal" political processes, group theory willingly ceded to other disciplines the responsibility of analyzing social movements, social change, collective behavior, political violence, attitudes, political psychology, revolution, and so on. Much the same sort of myopia was attributed to latter-day pluralist interpreters of group theory.

Off to a shaky start, group theory never really got off the ground. Even David Truman disavowed any claims for a broad-gauge theory. Not only did empirical research reveal a puzzling absence of groups from the political process, but also the theory itself became intertwined in the debate between pluralists and elite theorists, an unending debate. Pluralism, a reformulation of group theory, was alleged to be a defense of the status quo. Pluralists argued, according to Garson, that the process of accommodating the competing needs of groups was a satisfactory alternative to widespread public participation. Not only was this reformulation of democratic theory intellectually offensive to its critics, but there was also the nagging problem that very few of the groups whose interests were ultimately reflected in policy could be located. As Garson puts it: "Comparative research . . . tended to show that groups played a much more modest role than might be supposed on the basis of group or pluralist theory" (p. 96). Where, then, were these informal mechanisms of stability and consensus? Less important than group theorists believed them to be, they were also less benign. Most groups did not represent the views of their members accurately, as most people joined organizations for nonpolitical reasons and were uninterested in the allocation of political resources. Those groups that were politically successful tended to be those with a small, clearly defined, usually economic interest.

On the basis of the evidence he examined, Garson concluded that certainly by the 1970s, group theory was "anachronistic" and "well along its process of disintegration" (p. 177). Did Garson go too far? One can hardly read news magazines these days without encountering discussions of "single interest" groups, such as the various pro- and anti-abortion organizations. It is regarded as "true" that the National Rifle Association has, by dint of sheer persistence, prevented the passage of meaningful gun control legislation. The president has on several occasions lambasted the "oil lobby." As party identification declines, interest groups have increased the electoral activities to the point where they outspend political parties. One wonders, naturally, if Garson wishes he had not pronounced the corpse as dead before the vital signs were extinguished.

I suspect that Garson's answer can be inferred from his last two chapters. His definition of group theories is really quite eclectic. Thus, he spends a substantial amount of time discussing the works of scholars not generally regarded as wedded to the restrictions of group theory. Such writers, especially those interested in distinguishing between levels of policy, argue that there is a symbolic political process which occasionally attracts the attention of ideologically based groups. However, there is also a more significant process of decision which requires the use and exchange of technical information. This process is one largely under the control of bureaucracies which can dominate the information process. Sporadic outbursts do not really do much more than nibble at the fringes of the process whereby scarce resources are allocated. Much the same argument is advanced by Alvin Gouldner in his *The Future of Intellectuals and the Rise of the New Class* (Seabury, 1979), a work which, unfortunately, appeared too late to be included.

The book concludes with an attempt to be more constructive. Having systematically destroyed a once-popular theory, Garson felt the need to offer an alternative. Naturally, since it is easier to attack than to construct an acceptable theory, the results are less satisfactory than the main portion of the book. Garson offers a somewhat eclectic version of "political economy." He maintains that other contenders (public choice, for example) cannot claim as much support within the profession as did group theory at its peak. Why theories need to be popular to be correct is not clear. Nevertheless, Garson argues that public choice theory is "too conservative and economically simplistic" to be of much help. Other theories, such as systems theory, are "even more in eclipse than group theory itself. . ." (p. 191). "Critical social theory," a term Garson used to include a variety of perspectives ranging from critiques of pluralism through Marxism to various other more radical interpretations, is "not a theory but rather an orientation toward theory" (p. 192). So far, so good. But what of political economy, the "core concept of critical social theory?" (p. 192). Although this core concept is put forth as constituting the basis of the "new political science," it leads Garson to conclude that "If America is elitist, it is elitist in a pluralistic way, or, if pluralist, then pluralist in a way that benefits an elite. . . . America is neither a participatory democracy, nor is it dominated by a political-economic elite. It is, nonetheless, an elitist political economy." What does this mean? Is it clearer than the theory it seeks to displace?

There are a few minor problems. Good editing would have eliminated the occasional repeated point, for example. Other than this, and the interesting though flawed last chapter, the book does what it sets out to do in a highly readable way. It deserves to take its place among the standard works in the field. Ironically, Garson is so convincing in his criticism that the field may not survive.

HARMON ZEIGLER

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Justice and Reverse Discrimination. By Alan H. Goldman. (Princeton, N.J.: Princeton University Press, 1979. Pp. x + 251. $16.50, cloth; $3.95, paper.)

This is a carefully done, tough-minded, narrowly focused essay in applied social philosophy concerned with the justice of preferential treatment for minority group members and women in job hiring and school admissions. Goldman's conclusions are clear, frequently controversial and, as he wryly notes, likely to be equally distasteful to defenders of the status quo and activist reformers representing women and minority groups.

The book begins with a lucid introductory chapter setting forth the purpose of the study, definition of terms, and a brief discussion of the forms of moral reasoning used in the book. This discussion (together with the discussion of utilities and rights in the fourth chapter) should be particularly useful to those who come to the book with a greater concern for justice in public policies than familiarity with the technical intricacies of reasoning in moral philosophy.

Goldman relies heavily but not exclusively on a Rawlsian contractarian moral framework for generating fundamental principles. He also reasons by analogy, moving from particular cases on which there is general agreement to difficult cases where agreement is lacking. He is predictably critical of arguments to utility and interest that are common in questions of public policy. Justice in the subject which he is discussing is a matter of individual rights.

The argument begins with a reasoned defense of hiring by merit which is directed against libertarian and egalitarian attacks. Generally, he is somewhat more successful against the first than the second set of opponents. Society, says Goldman, may enforce a rule for choosing the most competent candidates for desirable positions because such a rule and the prima facie individual rights which it establishes advance the public welfare and protect individuals from arbitrary denial of equal opportunity. For those positions which require "open-ended degrees of competence" (p. 9) rather than merely a necessary minimum level of competence to qualify, the appointment-by-merit rule is fairer than random methods of choice called for by egalitarians. "Hence the rule is applicable principally for awarding positions in the professions, in graduate and professional schools, and to some degree in undergraduate colleges" (p. 65).

It is with such positions "that offer above-average reward or respect" (p. 30) that Goldman is concerned. Although positions which require only minimal competence are likely to be much more numerous, they get little attention in his discussion. No attention at all is given to how this distinction is, or should be made. Since the classification—open-ended or minimum-level—will often be far from automatic or self-evident, and since the merit argument seems much weaker where only minimal competence is required, the appropriate process for such classification decisions surely deserves examination. Unfortunately, such decisions are taken as given outside the scope of the analysis.

The second stage of the argument examines the extent to which reverse discrimination may be properly used to compensate for past violations of rights under the rule of competence. Goldman concludes that reverse discrimination is justified to compensate for violations of individual rights. With rare exceptions, group rights to compensation can only be justified when the harm of discrimination "cannot be analyzed into separate harms to each individual apart from his being a member of the group" (p. 83). Group liability is effectively denied. Reverse discrimination as compensation is owed only to those individuals who have suffered direct, clear and measurable harm. Given such harm, the right to compensation overrides an individual's prima facie rights under the competency rule. The extent of such justifiable compensatory discrimination would go well beyond current practice. Both public policies that facilitate group claims, *and* public policies which fail to meet the demands of justice for the individuals involved reflect mere political expediency. However advantageous to the politician, what is enforced by "political muscle" is not thereby made just. Like an earlier philosopher, Goldman seems to find justice and contemporary politics moving in different directions.

The third part of the argument turns to the future. When may reverse discrimination be justly used to bring future benefits? Goldman concludes that it is only justified when it will give socioeconomically deprived individuals equal opportunity to compete for positions under the rule of competence. He rejects the claims of groups defined solely by race or sex. Such group-based claims do not specify precisely enough those deprived of equal opportunity.

The final section applies the position Goldman has elaborated to an evaluation of current policies of affirmative action. Although committed to active policies of strong reverse discrimination on behalf of appropriate individuals, Goldman is predictably critical of programs which grant preferences to groups defined in terms of race or sex alone. Such programs miss the evil they are supposed to remedy and create new injustices: "As is usual in our political system, the poor and powerless continue to lose out, even when it comes to socially remedial policies. . ." (p. 223).

What can be said in criticism? First, although Goldman is arguing a case, he is clear, cogent, honest, and remarkably thorough. Many arguments which others have made are given a hearing at one place or another in his discussion. And one need not agree with Goldman to profit greatly from his reasoning.

Despite its many and genuine virtues, however, the analysis is narrower and more constricted than it might have been—or needed to be. Goldman is surely aware that he writes from a tough-minded commitment to a highly competitive notion of personal excellence, but there is very little (if any) explicit effort to look beyond this perspective and consider how it bears on his argument and conclusions. As suggested above, more attention might have been given to problems raised by that host of jobs which only require a minimum level of competence—and how they are identified. Further, although the questions with which the book is concerned have been forced on our collective attention by politics and political action, one would never guess from this analysis that politics could contribute anything but distraction and hubris. "Political muscle" and political systems where "as is usual . . . the poor and powerless . . . lose out" (p. 223) are not the only realities of the political context of justice. It is possible that contemporary politics contains some constructive possibilities for the realization of justice which intellectual rigor *and* imagination might be able to identify.

LANE DAVIS

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The Crisis of Political Understanding: A Phenomenological Perspective in the Conduct of Political Inquiry. By Hwa Yol Jung. (Pittsburgh: Duquesne University Press, 1979. Pp. xvi + 256. $18.00.)

According to Hwa Yol Jung, phenomenology is a "revolution or a new paradigm" (p. xiii) in man's self-understanding both as actor and thinker in the world. But despite the fact that phenomenology "seems to have finally come of age in the social sciences" (p. xiii), political theorists have largely ignored it. This "first systematic treatise" (p. xiv) of the phenomenological approach to political inquiry is intended to help dispel this ignorance and, incidentally, help drive another nail into the coffin of behavioralism.

The phenomenological "movement" is nourished mainly by the basic disposition and specific insights garnered from the disparate philosophical investigations of Husserl, Heidegger, Mearleau-Ponty, and Alfred Schultz. Phenomenology "claims to be a complete philosophy of man and of social reality" (p. 174), capable of relieving the "tension" between theory and practice which has troubled political theory ever since its Greek origins. This huge task phenomenology purports to solve on the basis of its own radical philosophy of "subjectivity": its main premise is the claim that "man is subjectivity" (p. 170), that the truly human is the subjective. By accentuating and exploring the subjective, phenomenology makes itself the true "guardian and servant" of humanity (p. 173). But man's subjectivity or humanity expresses itself for the phenomenologist in the radical sociality of man, a central and fundamental "ontological datum of his life-world (Lebenswelt)" (p. 60). In fact, in order to properly understand man's subjectivity, one must see it in the context of his life-world. The "primary task" of phenomenology is to discover the "genesis of meaning in man's life-world" (p. 62). Thus, man's humanity is most properly described in terms of a comprehensive philosophical anthropology based on an ontology of his life-world. Consequently, the phenomenological task of political science, too, is to "discover the genesis of meaning in the immediate, experiential, and common-sense knowledge of political actors" (p. 62).

The author sees his work as a "prolegomenon" to a phenomenological critique of politics as a new way of thinking about politics, or a "new angle of vision" (p. xv). Phenomenology, as the "vigilant activity of reflexivity" (p. xiv) that extracts meanings and truth from man's everyday, experiential life-world, offers a radical alternative to the ontological objectivism and epistemological scientism the author sees prevailing in today's theories of politics. This prolegomenon to a phenomenological critique of politics is expressly intended "to introduce phenomenology to those political scientists who wish to be self-conscious of what they are doing" (p. xiv). It is clearly not a book for political philosophers who are intent upon a deeper inquiry into the philosophical issues posed by modern phenomenology's key concepts such as man's subjectivity, his sociality, and the ontology of his experiential life-world.

HORST MEWES

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The Signature of Power: Buildings, Communication, and Policy. Harold D. Lasswell with Merritt B. Fox. (New Brunswick, N.J.: Transaction Books, 1979. Pp. xiv + 234. $29.95.)

Harold Lasswell had a good, sharp last look at a world he knew he was leaving. What he made of what he saw is the subject of this posthumously published book. He begins by saying that architects, planners and engineers make physical changes in the environment for political purposes and these changes influence the symbolic outlook and the behavioral activities of the people who adapt to their existence. His purpose in exploring "the complex interplay between the material and the symbolic" (p. viii) is to put the symbolic complexity of the way we interpret an altered environment to the use of the policy scientist.

When Harold Lasswell introduced American social science to what he called the policy perspective, he began by saying that the battle over method had been won. Policy was the new frontier. It meant using all that had been or could be learned about behavior to invent a future in which human dignity on the widest possible scale is realized. Environmental design can contribute to the goal because "one of the most profound and long-term connections between power and environment is the discovery and confirmation of the self. The self-system of an individual is composed of the primary ego symbol (I, me) and of the ego symbols (we) included with the primary ego to constitute and identity. Environments provide experiences that both separate and unify the self-systems of those who are exposed to them" (p. xiv).

In the past, political communication reflected "a divided and militant" world where tendencies toward segregation of the self triumphed over self-integrating tendencies. Lasswell believes that the physical environment can be shaped to communicate cooperation and interdependence. It is necessary "to replicate on a global scale the self-identifying function that has so often been accomplished on a small scale for neighborhoods, cities, nations, and empires" (p. 61). The earth itself can be transformed into an iconic form that evokes experiences of common identity and common values and goals. It can, as the measure of man, provide the unifying purpose that has traditionally been the province of the divine.

The social planetarium is recommended as a means for inculcating an appreciation of the iconic function of the earth. Using the technique developed by astronomers, models and maps of the earth would serve as the background against which scholars replay past trends and project future developments in alternative versions. Continuing decision seminars for policy makers could be conducted at strategically located social planetaria.

To have the environment communicate fundamental truths about man's state and the state he might create, albeit in alternative visions, it is necessary to know more about environmental design as an instrument of political power. Lasswell's model of the decision process is applied to the question of how buildings and other constructions are differentiated according to his decision phase categories. One hundred twenty pages of photographs of buildings, taken by Lasswell on his travels, are keyed to these analytic distinctions and "are intended to suggest some research opportunities compatible with the generalizations put forward. . ." (p. 83). Also included as a silhouette analysis (height with or without consideration of whether the building is on high ground) which ranks the structures according to the value-institution categories that constitute the Lasswell model of the social process.

Each of the pictures is the subject of Lasswell's sometimes droll commentary. Of a particularly run-down residence which has had a huge chain-link teller's screen tacked across the front and is thus transformed has become the Finance Department of Bloomington, Indiana, he says: "Occasionally, a converted building is rather superficially differentiated and demarcated as a means of heightening the status of the institution within. The State Finance Department office in Bloomington, Indiana, shows how a little money can go a long way" (p. 96). The License Bureau of Crawfordsville, Indiana, transformed from its former status as the town movie palace only by an "Auto License Branch" sign hung over the marquee, "speaks for itself"' (p. 96).

The research opportunities promised in the introduction to the exhibits are somewhat less forthright. Silhouette analysis seems to suggest little more than an update of Henry Adams' "Virgin and the Dynamo." Henry Adams compared the cathedral of medieval France to the machine of the modern age, concluding that each was the most appropriate symbol of its society's aspirations. It is necessary to go beyond the obvious fact that business buildings are usually the tallest in the town. The question is whether testable propositions can be derived from the connection between the degree to which power is shared and the patterns of environmental change.

Lasswell's architectural analysis of the policy-making process might be very useful in conjunction with the work Francine Rabinovitz has done on the relation between the cohesiveness of leadership groups and planning outcomes among communities (*City Politics and Planning,* Atherton, 1969). What building is placed where and is used by whom could tell of the "other face of power" (Peter Bachrach and Morton Baratz, "The Two Faces of Power," *The American Political Science Review* 57: 947–52).

Lasswell's assertion that architecture is best understood as the deliberate designing of environmental enclosures and thus narrow sharing of power favors an architecture of exclusion, and wide sharing encourages permeation, might provide an unobtrusive measure of power relations in the grandest sense of the irreverent terms of Webb et al. (*Unobtrusive Measures: Nonreactive Research in the Social Sciences,* Rand McNally, 1966). The case of the Philadelphia police headquarters is illustrative. Its architect and the mayor, his client, had something to say. A transparent glass facade held many doors invitingly open to the community. A subsequent mayoral administration, having won office on a law-and-order campaign, closed off all access to the front of the building and installed shades.

But architectural evidence of power relations is limited. For one thing, the building-by-building analysis of the decision functions of government omits an entire sphere of activity in that the wires used to tap telephones do not perform a visibly enclosing function. Lasswell was aware of this weakness. He said an architecture of communication does not exist because the function has been taken over by engineers. If we allow that what people say about what they are doing may or may not have much relevance to what they are doing, we would still be confronted by the inexact state of the science of reading accurately what is said by physical arrangements. This is not so simple a task. Attempts to apply the principles of semiology to an understanding of architecture have not been very successful. (See, for example, Charles Jencks and George Baird, eds., *Meaning in Architecture,* Braziller, 1969.)

Reading the environment for clues to power relations is secondary to Lasswell's purpose. He is most interested in using the environment to influence behavior. In the prodigious scholarship brought to the work, he neglects those sociologists who have studied the influence of physical arrangements on behavior. Robert Gutman (*Buildings and People,* Basic Books, 1972) and Herbert J. Gans (*People and Plans: Essays on Urban Problems and Solutions,* Basic Books, 1968) have been critical of what they consider facile theories of the social influence of architecture. Gans looked at planning as a method of public decision making and concluded that architectural and site planning and most policies which seek to change the physical environment have had little influence on the behavior and values of people.

Yet Lasswell says, "The modified environment . . . keeps alive the fundamental perspectives of the world political system: the expectation of violence, the emphasis on national identity, and the demand for political power. . . . We are surrounded by redesigned environments that keep alive the message that 'we' must be ready to exclude by the use of force those who would break into, or insinuate their way into our national enclosure" (p. 55). Given the ratio between our population and the portion of world resources Americans enjoy, there is perhaps merit to secure enclosure. Harold Lasswell assumes beneficence. He assumes that his social planetaria will be lessons in brotherly love. The assumption that others will react to a given environment the way that Harold Lasswell would react needs to be explored more carefully.

LINDA LOUISE SCHLUTER

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Locke, Hobbes, and the Federalist Papers: An Essay on the Genesis of the American Political Heritage. By George Mace. (Carbondale: Southern Illinois University Press, 1979. Pp. xiii + 162. $12.50.)

This addition to the growing literature on Thomas Hobbes and the American founding focuses on the relative contributions of Hobbes and John Locke to the Declaration of Independence and *The Federalist.* The method employed by George Mace is textual analysis. His purposes are to demonstrate a closer affinity of the American political heritage with Hobbes than with Locke, and then to demonstrate the superiority of Publius to both. An assumption of the analysis is the "wholly" natural-rights nature of that heritage which is reposited in the Declaration (pp. 7–8). Hobbes is seen as more relevant to that document's claims, even though Mace quotes Jefferson's attribution to the "sentiments of the day" found, among other places, in such books of "public right, as Aristotle, Cicero, Locke, Sidney, or &c." (p. 9). Hobbes is introduced as part of the "&c." In light of the positive attitudes toward Locke found throughout writings and documents from colonial times and during the nation's early years, claiming Locke's displacement by Hobbes as the theorist deserving credit for "defining the birthrights in our heritage" (p. x) is a tall order for a book of only 120 pages of text about texts.

A survey of the multitude of authors who were cited and discussed by the many individuals involved in constructing the new nation indicates that no single theorist ought be given sole credit for defining our birthrights, especially not Hobbes. The reason for taking up this task seems entailed in a desire to replace the arguments that we are a Lockean nation with the idea that we are a "Hobbesian nation" (p. xi). To establish this claim Mace presents brief chapters on the natural-rights character of Jefferson's Declaration (Ch. 1), a denial of Locke's affinity with it (Ch. 2), four views of Hobbes' "natural rights" theory (Chs. 3–6), and a conclusion that Hobbes is closer to Publius in *The Federalist,* though the latter is superior (Ch. 7).

Mace nowhere demonstrates an actual use of Hobbes to balance against Locke's and Jefferson's similar structures of argument, style, and content. Indeed, Mace narrows his focus to "inalienable rights" without similar analyses of other important concepts, such as "consent" and the "public good." The parallel arguments on the right of a public to alter or abolish a government destructive of rights and the public good, employed in both Locke and the Declaration, are not traced. The Lockean nature would not disappear even if one views Locke's "property" and Jefferson's "pursuit of happiness" as decisively different. Indeed, many scholars doubt the purely natural-rights character of any document employing religious support.

Mace introduces an interesting and complex argument that the end of Hobbes' theory is happiness, as a non-wearisome life, which becomes an inalienable right beside the "right to life" (pp. 56–63). Mace sees Hobbes' non-wearisome life as closer to the pursuit of happiness than Locke's right to property—including property in one's body. Even if this is superior to previous readings of the *Leviathan,* it is never demonstrated that any Founder understood Hobbes in this fashion. The chapters interpreting Hobbes are worth serious consideration, but Hobbes' impact in America cannot be assessed through this controversial interpretation. Without evidence to the contrary, it seems more probable that Americans saw Hobbes as a justification of sovereignty, not of inalienable rights. Mace's claims are not susceptible to demonstration by means of his method.

The claim to illuminate the source of the American political heritage by a single theorist misses the genius of that heritage. Indeed, the structural flaw of the book—moving from 1776 to 1787—88 as if the views of Americans were unchanged—illustrates the complexity of the underlying confluence of ideas and experiences. Nevertheless, Mace's interpretation of Hobbes is worth attention because it departs markedly from standard interpretations.

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Marx's Theory of Politics. By John M. Maguire. (New York: Cambridge University Press, 1979. Pp. vii + 251. $28.50.)

One of the ironies of our time is that people who teach and write about political theory seem often to forget that "politics" and "theory" are themselves problematic terms. Thus, John Maguire entitles his book *Marx's Theory of Politics* without ever indicating that Marx's conception of "theory" and of "politics" rested on an attempted reversal of how those terms are understood in the classical writers, for example. The result is a rather bland account of parts of Marx's teaching torn out of context. Part of the time the author seems to use "politics" as if it were a self-evident, commonsense term in contemporary Anglo-American discourse. In other sections he purports to show how Marx used "politics" (in comparison with "government" or "administration") to symbolize that condition of things prevailing in class (and especially "bourgeois") society. In the latter sense, primitive communism is "prepolitical" and final communism "postpolitical."

To be consistent, Maguire should have entitled his book *Marx's Comments on What He Called "Politics."* To aver that Marx had a "theory of politics" is to impose a language foreign to Marx himself, who insisted on the reality of *praxis,* or the dialectical unity of "theory" and "practice." For Marx, it would have been a merely "academic" exercise to put forward anything like a "theory" of "politics" separable from the rest of his work. That work stood or fell as a whole.

In any event, Maguire insists that Marx did *not* achieve "a comprehensive and definitive viewpoint" about politics. What, however, is a theory but an attempt at a "comprehensive viewpoint"? Rather in the fashion of a faculty member correcting the first draft of a graduate student's dissertation, Maguire frequently "grades" Marx on how well or poorly he has done in this or that remark about "politics." In this instance, however, one wonders whether it was Marx who failed to achieve a "comprehensive viewpoint" or whether it was Maguire who failed to understand what, to Marx, a "comprehensive viewpoint" entails. I suspect that Marx would have flunked his "grader" for interpreting his work by the alien standards of "bourgeois" academic thought.

Maguire would not have failed Marx, however. Indeed, he would have given Marx high marks. Maguire concludes the book with the statement that Marx left us "an understanding of the nature [!] of politics . . . which . . . is suggestive and profound" (p. 242). (Marx, of course, would deny that politics had a "nature." To understand why would entail a thorough examination of his "philosophy." Maguire insists on setting Marx's philosophy aside however.) By what criteria does the author make this statement? What is *the author's* theory of the "nature" of politics? Alas, for that one searches in vain. Thus, the book's opening statement that "this is a study of how Marx treats politics" is false, unless one means to accept uncritically Marx's confinement in some passages of "politics" to "class society." The author does not accept Marx's confinement of "politics" to class society, however (although he does suggest that Marx's view has more merit than is commonly supposed). For Maguire's book to have been intelligible, he would have needed to preface it with what he understands to be a critical theory of politics. Even if he did this, he would then need to attempt to enter into Marx's thought on Marx's own terms to show what politics means in Marx's teaching. Then he would have to evaluate the *entire* Marxist teaching about human beings, society, and history, in which Marx's treatment of "politics" is embedded.

In summary, the design of Maguire's book is fatally flawed. Some scholars may find it useful as a convenient repository of quotations in English from Marx's many works. The background information about the upheavals of 1848 and 1870—81 is also useful. The claim of the Cambridge University Press that "this is an important book because of the exceptional combination of historical and theoretical perspectives Maguire brings to the examination of

Marx's theory of politics" is dubious, to say the least.

DANTE GERMINO

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Utopian Thought in the Western World. By Frank E. Manuel and Fritzie P. Manuel. (Cambridge: Harvard University Press, 1979. Pp. vi + 896. $25.00.)

There is much to admire in this massive book. Its sweep from visions of the biblical paradise to the ideas of Marcuse and Skinner is impressive, even dazzling. Its organization is surprisingly tight. The authors skillfully weave a unified book from what must be, even in this long work, a necessarily partial survey. *Utopian Thought* is also appealingly rich in textured facts and analysis about its subjects and it is obviously rooted in scholarship of near-heroic proportions. Finally, one must note the splendid style of this book. The Manuels are obviously writers, and it is a delight to read their work.

The most impressive individual parts of *Utopian Thought* include the polished results of studying the utopians the Manuels know best, "the prophets of Paris," from Saint-Simon to Fourier; fascinating sections detailing Renaissance utopians, including those with modest reputations such as Pastor Andreae and Giordano Bruno; their journey to Rousseau, an essay in appreciation of his work as "a prophetic . . . and maybe even true, discourse on the state of man" (p. 452).

The list could easily go on, because so many parts of the Manuels' distinguished achievement deserve praise. At the same time, one may note a few reservations without diminishing the luster of their accomplishment. The Manuels' choice to describe *individual* utopians and utopias, electing to avoid systematic investigation of utopia as a form of life, the utopian perspective as a cast of mind, or the dialectic between utopian ideals and history, is troublesome. *Utopian Thought* offers too few testable hypotheses or even speculations which social scientists might test to develop theory about the utopian adventure in human experience.

An immediate consequence is that the Manuels do not much develop or defend their rationale for including (or not including) particular thinkers. They have a predictable and defensible (but not defended) bias for the French tradition, sometimes to the extreme. For example, the only anarchists given any real attention are Proudhon and Sorel. They also prefer the utopian thoughts of great philosophers to those of others who were sometimes more influential and popular. Were Kant and Leibnitz more significant utopians than Bellamy? Not surprisingly, the Manuels also largely ignore American utopian theorizing.

A second problem in *Utopian Thought* is its frequent, undefended normative biases. Three in particular recur again and again. One is an animus toward Christianity (or all religion), predictable in devoted students of the French Enlightenment. The authors suggest that "religious zeal has been the most barbarous of human vices" (p. 144). In its more enthusiastic forms, such as Anabaptism, religion is nothing more than "the deception and madness of paradisical fanatics" (p. 183). Even the consequences of Christianity's intellectual puzzles are ugly to the Manuels. "Christianity with its trinitarian definition of God had [an] . . . irksome intellectual problem . . . which cost hundreds of thousands of lives" (pp. 624–25).

The Manuels dislike all believers, for *Utopian Thought* is also pointedly unsympathetic to Marx and Marxism. They boldly label Marx and assorted Marxists as utopians, an insult to the faithful; they show no warmth in their considerations of Marx or Marxism—though the Manuels avoid polemics; they repeatedly fault Marxist historians for abusing, even falsifying, the history of ideal societies for partisan political ends; they directly attack modern intellectuals for listening too much to the siren calls of Marx (p. 713).

Finally, the Manuels often appear to take neither utopias nor their proponents seriously on their own terms. This is disturbing. They are much more interested in speculations about the psychological roots of thinkers and ideas than one would like. While their insight in this task is sometimes modest, their psychologizing is relentless. For example, they inform us that Francis Bacon's rebellion was limited because "to strike out against the political order would mean facing his father and to cast doubt on religion was an assault against his mother" (p. 250). Or they tell us that we know Bacon was a homosexual partly because he denounced homosexuality and thereby engaged in "a classical form of sexual self-revelation" (p. 245). Sexual observations are a constant theme. Fourier's writings show that he was a "masochist" with "perhaps homosexual elements that are not infrequently accompaniments of sadism" (p. 674). And, of course, Rousseau's thought offers a field day for these speculators about sexual motivations.

Despite these problematic features, *Utopian Thought* is a majestic book in many ways. While the Manuels share the spirit of our age and view utopias without love, as Diderot did, as "distant, cold, general," places where "nobody jokes" (p. 424), no one has written more authoritatively about utopias or their creators. We can be confident that no one will soon match their achievement.

ROBERT BOOTH FOWLER

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Injustice: The Social Bases of Obedience and Revolt. By Barrington Moore, Jr. (White Plains, N.Y.: M. E. Sharpe, Inc., 1978. Pp. xviii + 540. $17.50.)

"Why political revolution?" is surely as important and fundamental a question as any that political and social scientists confront. Barrington Moore's speculative but empirically rooted reassessment of that question will enlighten and educate all of us on it, whether or not we happen to be subscribers to (or even authors of) one or another of the mechanically deterministic macro-level theories about socio-economic causality or the ahistorical, two-dimensional, stick-figure micro-level theories about individuals' "relative deprivation" or "distrust" which Moore gently, and often indirectly, rejects in the course of developing his own broad working hypothesis.

The work is a model of social research in several respects. It seeks and marshals a formidable array of empirical evidence and data, historical, documentary, biographical, and behavioral as appropriate to the questions at hand. It can be called a "behavioral social science work" in the best sense of that term, in that it moves back and forth along the "levels of analysis continuum" with ease and grace, never losing sight of the crucial orienting macro-level questions, yet never failing to examine the dynamics of micro-level behavior that compose the macro-level phenomena under investigation.

It is also a "theoretical work" in the best sense of that term, i.e., not as a set of statements "about theory" or theories, but as an effort to formulate general propositions about the subject of the inquiry. Its concerns are both empirical and normative. It examines not only questions about the origins and course of political revolutions and rebellions, and the actions and behavior of people in them, but questions about the moral judgments embedded in the actors' actions, including questions about the probable truth or falsity, if not the absolute validity or invalidity, of the moral judgments they make and the moral standards on which they base them.

The nature and shape of Moore's argument can be sketched briefly, even though the sketch will hardly do it justice. A clue to his distinctive and crucial contribution lies in the book's subtitle, "the social bases of *obedience* and revolt." Alone among the major scholars dealing with revolution, Moore begins by recognizing that any explanation of why people do not obey political authority must not merely include but start from an understanding of when and why they do. He begins, moreover, with a fundamentally biological working hypothesis, holding that the answers are to be sought in "the interaction between more or less constant aspects of human nature and equally recurring imperatives that stem from the fact that human beings must live with each other, that is, in human society" (p. 5). And he explicitly recognizes that any conception of some "innate human nature," to the extent that it is valid, "implies the existence of a 'natural morality,' in the sense that *some* moral preferences . . . are not merely the consequence of social training and conditioning" (p. 7).

In part 1, Moore combines insights from Stanley Milgram's neglected experiments on the bases and limits of obedience to authority with logical inferences from the social imperatives of division of labor and distribution of goods and services to arrive at his principal hypothesis: the key element in political and social rebellion or revolution is "moral outrage" at authorities or other social groups who violate or seek to change the terms of the fragile, shifting, but normally prevailing "social contract" which embodies people's understandings about right and justice in these matters in each particular society. An interesting and unusual part of the argument here is heuristic discussion of the submissive behavior of ascetics, Hindu untouchables, and inmates of Nazi concentration camps.

Part 2, the central half of the book, is devoted to historical events and socioeconomic and demographic developments in Germany from 1848 through 1920, with special attention to the behavior of German industrial workers in the light of the major hypothesis developed in part 1. In the course of this presentation Moore diverges substantially from Marxist and other familiar interpretations of the revolutionary process allegedly developing throughout these several generations.

Part 3, the last quarter of the book, broadly and sketchily compares these events with comparable developments in Russia of the same period, again with substantial divergence from most commonly accepted interpretations. It goes then into the question of the "moral responsibility" of Friedrich Ebert and Social Democratic leaders of the early 1920s for the choices they made after World War I and for the sad events which ensued in Germany a decade later. Moore returns finally to very general development of the thesis about "moral outrage," and to highly abstract empirical and normative speculation about its origins and consequences. Most readers will probably find the concluding discussion disappointingly abstract and truncated as a summing up of what has gone before.

The important point, however, is not that Moore stops short of producing a "definitive" treatment of his powerful and provocative working hypothesis. That the mode of argument and presentation is dialectic and didactic rather than rigorous and comprehensive should not mislead one into underestimating the critical inroads Moore has made into prevailing social-science conceptions of the events he analyzes, or the extent to which his working hypothesis opens up and broadens out our conception of the human behavior and problems under examination. Whatever the perspective or the level of sophistication and knowledge from which one starts, the careful reader of *Injustice* is bound to come away with greatly enhanced understanding of revolutionary situations and the actors in them—their motives, the alternatives really open to them, the reasons for their choosing one course of action over others, and the social and cultural consequences of their collective choices and actions.

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Aggressive Political Participation. By Edward N. Muller. (Princeton, N.J.: Princeton University Press, 1979. Pp. 305. $17.50.)

Most recent research on unconventional political behavior focuses on macro-level conditions that tend to be associated with domestic conflict and violence. Hence, this book, which develops and tests a micro-level theory of aggressive political participation, is a welcome addition to the literature. Although it is based on surveys conducted in West Germany in 1974 and 1976, this book is not about Germans; rather, it represents an effort to build a generalized theory about unconventional forms of political behavior.

Edward Muller labels his explanation for aggressive political participation the Expectancy-Value-Norms theory. He argues that aggressive political participation is dependent on the individual's expectation that such behavior will have "beneficial consequences"; "the value or utility of aggressive political action to the individual"; the extent to which the individual's "personal normative beliefs" justify such behavior; and the extent to which the individual is exposed to facilitative "social normative beliefs" (p. 29). To operationalize aggressive political participation, Muller constructs an index which considers actual behavior as well as intention. Moreover, he weights each of the behaviors included in the index by the amount of social disapprobation associated with it. For the most part, the operationalization of the predictors is accomplished by combining the scores on a series of more or less standard scales. For example, Muller builds a Normative Justification for Aggression (NJA) index by multiplying the scores received on an 8-item Political Support-Alienation scale by scores received on a Left Ideological Commitment scale.

With all the variables in the equation, Muller finds he can explain almost 57 percent of the variation in aggressive political participation with his theory. While this is a "satisfyingly high level of predictive accuracy" (p. 120), Muller proceeds to see if he can improve on it by including other variables. After analyzing the impact of such things as frustration (just deserts) and policy satisfaction in chapters 5 and 6, he concludes he cannot. In the final three chapters of the book, Muller checks the cross-validity of the prediction equation, discusses the uses and limitations of the theory, and postulates—but does not test—the potential links between the components of his model and certain macro-level conditions.

There is much of value in this book. Particularly impressive is the close link between theory and data. Muller discusses all of his operationalizations as well as the theoretical rationales for them in the text. His procedure represents a fine model for the reporting of empirical research. The rigor of Muller's analysis as well as his index of aggressive political participation are also impressive. Finally, this book has value because of the goal it seeks to achieve, namely a generalized theory of the roots of aggressive political participation. Although it is likely that other researchers will take issue with some of Muller's postulates and operationalizations, it seems probable that this book will serve as the starting point for future theorizing and research about aggressive political participation.

Muller's analysis, however, is not without problems. Perhaps most aggravating is the multitude of indexes constructed to test the theory. Although Muller lists all the indexes and the chapters in which each is first introduced in Appendix B, the reader can easily get lost in the analysis. Muller's principal indicator of his principal concept, namely NJA, will probably also be subjected to some criticism. It is not clear that his Political Support-Alienation index is an effective operationalization of Easton's concept of support. To his credit, Muller devotes a long footnote at the very end of the book (pp. 261–62) to an alternative method of measuring this concept. Finally, since Muller's analysis indicates that aggressive political participation is strongly related to such standard variables as system support and age, some may argue that he has not told us anything new.

Overall, though, this is a fine book which makes a valuable contribution to the study of aggressive political participation. It has laid some important groundwork and there seems no doubt that it will stimulate substantial research in the future.

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John Locke. Edited by Geraint Parry. (Winchester, Mass.: Allen & Unwin, 1978. Pp. xii + 171. $18.95, cloth; $8.95, paper.)

The problem with those traditional thinkers whose views have infiltrated our cultural consciousness is, as Marx put it, that their ideas "weigh like a nightmare upon our brains." Within liberal capitalist society, John Locke's political thought has attained an apparitional status of considerable familiarity. Lockean ideas are linked with the major tenets of "modernity" (Strauss), capitalism (Macpherson), elitist liberalism (Seliger), and now they appear in Geraint Parry's new book in the company of "conservative liberals" such as F. A. Hayek and Milton Friedman. Clearly, Lockean liberalism is an entity sufficiently malleable to accommodate a large number of the nightmares experienced by our contemporaries.

For Parry, Locke has become the spokesman for "self-reliance" and "individualism." "Locke articulated, as clearly as any writer had previously done, the politics implicit in the 'theory of individuality' which had gradually emerged in Western Europe with the decline of feudal society. . . . Locke's concern was with the form of political association which would most effectively permit the cultivation of individuality" (p. 14). The idea of individuality, Parry argues, lies at the heart of Locke's views on epistemology, religion, and politics. Thus "the Lockean individual is one who can stand up against the pressures of authority and think his own thoughts, who is prepared to withstand the social consequences that heterodoxy might bring. He must see for himself, think for himself, believe for himself, and assent for himself" (p. 46).

With respect to the secondary literature, Locke, the individualist, is hardly a new Locke. Still, Parry is sensitive enough to recognize that Lockean individualism is neither as abstract as Sabine made it appear nor as "aggressive" as Macpherson contends. (Oddly, Parry uses this adjective throughout instead of Macpherson's term, "possessive individualism," which, in my view, presents more serious difficulties for Parry's interpretation.) This sensitivity derives, in part, from Parry's awareness of the importance of religion as a foundation for Locke's ideas, and, generally, from his efforts to pay attention to recovering Locke's intentions as an author as a necessary starting point for any interpretation of Locke's writings. Indeed, Parry rightly warns that "intellectual dangers" arise when interpretations of the "Lockean tradition"–of which there are many–are "confused with the historical inquiry into Locke's own meaning" (p. 151).

Nevertheless, Parry insists that Locke's political and moral assumptions must be severed from their religious roots if they are to retain "significance" for us (p. 14). At the same time, Parry wants to argue against that "legacy of Lockean liberalism" advanced by the "new liberal" advocates of social democracy and the welfare state on the grounds that the latter has fewer "affinities with Locke's aims" than the interpretive view he defends (p. 151). Now, this is perilous terrain, since once we have so easily committed an "injustice" against Locke's intentions by dropping his religious beliefs, the way is certainly opened up for others to reply that Locke's views on "poor relief," "self-reliance," and "private property"–to which, in "moderated" form Parry subscribes (p. 158)–are as outdated as are his religious beliefs in the context of contemporary society. Moreover, the problem of what determines the "significance" for us of *some* of Locke's ideas and not others, as well as what criteria one uses to evaluate the significance of competing "legacies" of Lockean liberalism, raises issues which are not directly confronted in *John Locke*. Yet Parry's preference for one legacy over others and his belief in its contemporary significance supply the purposeful structure of the book he has written, and necessarily presuppose the existence of such criteria, which he has employed in order to resolve these problems.

An interpretation of Locke's ideas, in other words, always points in two directions in its assessment of those ideas. It presents, in *some* respect, a contextual view of Locke's intellectual surroundings in terms of which the meanings of his statements are explicated, and it assumes some basis for the recognition by the reader of the relevance of these views. These undertakings, of course, may be carried out well or poorly in terms of the historical evidence presented and the conscious attention paid to the interpreter's presuppositions. Against the record of existing scholarship dealing with the history of political theory, Parry does reasonably well. He recognizes that placing Lockean ideas in their historical context is necessary if readers are to grasp the dimensions and the implications attached to the choices they face with respect to the "many and varied Lockean legacies," which, like the ghost of Hamlet's father, appear now and then to urge them to commit—or not to commit—some political action.

There is much to be said for both the relevance and the historical accuracy of Parry's Locke who "opposed authoritarianism in intellectual life" (p. 2). Certainly, a good deal of this anti-authoritarian perspective, especially in philosophy and religion, depends upon and reinforces the development of "individuality" as Parry describes it. Still, the darker shadows of this enlightenment liberalism begin to appear when we shift the meaning of Locke's "individuality" towards its identification with those who are "industrious," or its necessary connection with private property, leading to the conclusion that "a capitalist market economy is undoubtedly that which is most congruent with Lockean civil society" (pp. 42–43, 50, 123). The interpretive problems in *John Locke* arise for Parry because his view of Locke supposes that the meaning of "express consent" extends to all individuals and therefore beyond Macpherson's identification of that term with a defense of property rights, although Parry admits that he has no textual warrant for this reading (thus, he judges *Locke's* account of express consent to be "seriously inadequate") (pp. 103–08). At the same time, Parry argues for the importance of a "moral dimension" to Locke's linkage of private property with the development of "individuality" (p. 50). Yet when later liberals (e.g., T. H. Green) used *this* aspect of Lockean liberalism in defense of the view that society ought to "redistribute its resources to place the deprived in a position to cultivate their individuality," they are accused by Parry of having taken the wrong historical turn (p. 157). It is true that Locke did not assign this task to civil society, but it is far from self-evident that it does more violence to his intentions than the attempt to use his definition of private property (in its "moral" dimension?) in defense of the social and economic power of IBM, Standard Oil, or General Motors.

Throughout *John Locke*, Parry is concerned—almost obsessed—with arguing that civil society has no "right" to redistribute private property in order to lessen social inequalities (pp. 120, 153–56). This is a bit odd, since no one has attributed such a view to Locke. At the same time, Parry enlists the support of Hayek, Banfield, and Friedman in their criticisms of the ends and achievements of welfare state liberalism. There is nothing wrong with this, but to conclude, as Parry does, that "the remedy" for the political problems of contemporary society "must be to turn the state's activities back to something akin to those of Locke's civil government" (p. 158), assumes the construction of a bridge over troubled waters for which all the necessary materials are not provided in *John Locke.*

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The Problem of Political Obligation: A Critical Analysis of Liberal Theory. By Carole Pateman. (New York: John Wiley, 1979. Pp. xi + 205. $21.95.)

Subtle insight, originality, and the dogged pursuit of an idea mark Carole Pateman's new book on political obligation. "Obligation" for Pateman can only mean "self-assumed obligation" (pp. 12–13). This assumption leads her to conclude that only a fully participatory democracy—where each citizen either wills each law or actively consents to a majority willing that law—can create genuine political obligation.

Pateman's consideration of different versions of the competing "liberal" theory of obligation and the analogy she develops between political obligation and the social practice of promising form the strongest parts of the book. Hypothetical, or tacit, consent cannot produce political obligation, she argues, because " 'consent' is meaningless if people do not know that to perform a certain act is to consent" (p. 73). Neither can non-acts like electoral abstention count as acts of authorization that create obligation. Nor, if one adds the criteria of relatively free or conscious action, can voting in a polity in which the government significantly manipulates public opinion, or in which the policy consequences of voting are difficult to decipher, be said to imply consent. Finally, Pateman considers the claim that political obligation is a spurious problem rising from the individualist's inability to understand "what it *means* to be a member" of a policy, and concludes that this "conceptual" view "says nothing about specific rules or forms of social institutions" (p. 107).

What of the claim that we are politically obligated to obey just laws because they are just? Pateman distinguishes between actions we "ought" to perform because those actions are "generally right and morally worthy to perform" and actions we are "obliged" to perform because we have voluntarily assumed that obligation. In the first case, she argues, the citizen simply accepts or recognizes an already existing duty; only in the second case is there a freely created, public, "obligation" (pp. 28–29). This distinction between what we "ought" to do and the obligations we have voluntarily assumed is useful. But Pateman does not convince me that it is useful to restrict the term "obligation" to that which we have voluntarily assumed. It would be more plausible to argue that when one's voluntary assumption of obligation coincides with one's sense of justice, one's obligation is clear; when the two conflict, one's obligation is unclear.

Pateman does not claim to have developed a full theory of political obligation. She claims only that "the problem of political obligation can be solved only through the development of the theory and practice of participatory or self-managing democracy" (p. 1). Yet in the context of her interpretation of Rousseau, she puts forth the first tendrils of what she presumably believes to be a valid theory of political obligation. This is the weakest section of the book.

For Pateman (or Pateman-Rousseau), political obligation, though self-assumed, is not purely procedural (p. 92). The mere agreement of citizens does not (ever?) create political obligation. Rather, political obligation presupposes "substantive principles of political morality," like the principle (the only one Pateman spells out) that each law must "benefit or burden all citizens equally" (p. 152). If genuine political obligation is to be created, each citizen at the moment of the social contract must actively will some or all of the principles of political morality. Citizens come to will the principles of political morality whenever their previous experience of political participation (presumably under either "the liberal social contract" or some other imperfect regime) has had a sufficient "educative effect" for them to see the light (p. 156). Whole polities can revoke the principle(s) of political morality (p. 154), although perhaps individuals cannot (p. 82).

Individuals are, however, remarkably free in this participatory polity. Even within the social contract, not every individual is obliged to conform to the mandate of every law. In analogy with the social practice of promising, citizens incur political obligation only by knowingly committing themselves to their fellow citizens through their votes on specific issues (citizens must "understand that to vote . . . is to commit oneself" [p. 161]). Citizens who do not themselves vote on a specific law (e.g., by referendum) remain uncommitted. The minority who vote against a winning proposal also remain uncommitted unless they judge individually either (1) that the majority in voting followed the principles of political morality or (2) that at least the majority acted in good faith, thinking it was following the principles of political morality.

In short, the only polity that could bind its citizens would be both based on the principle(s) of political morality and a "truly voluntary association" (p. 155), or on the largest scale, a "political association of a multiplicity of political associations" (p. 174). Unfortunately, such a polity could not act when individual interests came in conflict. It could only act, as Rousseau said a polity *should* only act, when there was an overriding common good on a given issue. Indeed, if citizens were to take seriously the principle of equal benefit, their polity could seldom act even when there was a common good, because laws can seldom avoid producing more benefit for some than for others. Thus it seems unlikely that a polity based on the principles Pateman begins to sketch out here could survive.

Its low degree of practical applicability may lead readers to dismiss Pateman's fledgling conception of political obligation. So may her occasional overstatement or careless wording. Such a dismissal would be a mistake. Her attempt to push the analogy with promising as far as it can go, while not resulting in a convincing theory of political obligation, does permit a successful challenge to many of the shibboleths of the liberal tradition, and lends itself to intriguing interpretations of Hobbes, Locke, Hegel, Godwin, and others. It poses starkly problems that are central to developing a coherent theory of democracy.

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Émile or On Education. By Jean-Jacques Rousseau. Translated by Allan Bloom. (New York: Basic Books, 1979. Pp. ix + 501. $18.50, cloth; $7.95, paper.)

Unlike his Plato's *Republic,* Bloom here reserves extended commentary to another project, adding only a 25-page introduction and 14 pages of notes indicating Rousseau's sources and translating Latinisms, etc., inconveniently left standing in the text.

Adherence to Leo Strauss' goals of literalness and consistency strengthens this translation. Its superiority is obvious to anyone who has crowded the margins of the 1911 Foxley translation with corrections. Apart from omitting at least one paragraph which Bloom restores (his p. 272), Foxley loosely phrased even the famous opening of Book 1. Above all, Bloom avoids Foxley's exasperatingly inconsistent renditions of central terms. Rousseau's generic term for preoccupation with one's standing in the eyes of others is wisely left untranslated as *amour-propre* in Bloom but was obscured by a half-dozen English substitutes in Foxley. Joining sound translations of the political writings, Bloom's version of Rousseau's masterpiece diminishes handicaps for students with little or no French.

Strauss' influence weakens Bloom's introduction, which works the familiar pattern of magnifying contrasts of Ancients and Moderns by exaggerating consensus within each group. Rather than training rational souls to master bodily passions, the Moderns engineer environments to control results of passions which inevitably master reason, neutralizing what is nasty in the passions and anointing what is nice (these ground rights and derivative duties). Like Strauss and Roger Masters, Bloom thinks Rousseau's dualism of matter/body and spirit/soul is dubious or detachable. Objection: Dualism is necessary for the coherence of Rousseau's otherwise contradictory usages of "natural." As merely physical beings we naturally tend to subordinate general interests to the particular and exempt ourselves from general rules, but as spiritual beings we naturally ought to subordinate the particular to the general, sometimes even sacrificing our lives. But the soul's reason is motivationally barren and conscience is weak unless bodily passions of (sublimated) sexuality and (redirected) *amour-propre* become allies rather than antagonists of compassionate reason.

Bloom perversely views compassion as a "selfish passion" (p. 18), claiming that Rousseau merely derives duties from different passions than Hobbes' fear of death, which is beyond experience before conceptualized. Bloom should say that the *intensity* rather than the existence of self-preservation motivation is held unnatural (cf. pp. 9, 82). Bloom accents discontinuities more than continuities between a favored Plato and the democratizing Rousseau, but if Plato's sexual equality was ironical we expect Bloom to warm to Rousseau when clearly "sexist" (pp. 23–24). Bloom complains that Rousseau invented "the disadvantaged" by assessing claims on society by negative lacks rather than positive contributions (p. 18). His sexism aside, Rousseau could respond: the capacity to give to a society is a consequence of what one has got from it.

TERRENCE E. COOK

Washington State University

Marx's Method: Ideology, Science and Critique in *Capital.* By Derek Sayer. (Atlantic Highlands, N.J.: Humanities Press, 1979. Pp. xi + 197. $20.00.)

Derek Sayer's intent was to write a "clear book that would be accessible to more than a specialist audience" (p. x). Though a "detailed knowledge" of Marx is not a prerequisite of what follows, nonetheless Sayer concedes that some readers might prefer to pass over some of the "more technical discussions first time around" (p. x). Midway through chapter 2, "the Language of Commodities," the reader is confronted with the Marxist concept of the value of commodity expressed as (p. 27): Z com $A = u$ com $B = v$ com $C = w$ com $D = x$ com $E = \ldots$ etc. Unfortunately, there are some other instances (pp. 116, 168) of Marxist hierograms that contradict the author's stated purpose of writing a "clear book."

To compound the problem, the writing is rather obscure and jargon-laden. Imagine what would have become of Christianity if Jeremy Bentham had written the parables. One can sense the result by reading Sayer's book. The author should have remembered, facing his typewriter, the old tag of his Latin Grammar—

Masculine will only be
Things that you can touch and see.

Instead of simple English, the book is dotted with Latin phrases; Marx's method is *coupure* (p. 103); Marx's *explananda* differs from his *explanans* (p. 109); Marx's analytic is discussed in *seriatim* (p. 110); then *ipso facto* and *inter alia* intrude upon the reader; money-commodity is *simpliciter* (p. 41) as are "observable empirical regulaties *simpliciter*" (p. 114); the author even "bespeaks" of Humean "expiricist" (p. 114). This could mean "empiricist." But one no longer knows.

As for the substantive concern of the book, the central argument of Sayer is that Marx's method of enquiry is distinct from his presentation. The proposition involves what the author defines as unearthing the "intrinsic" relations behind the "external" movement of capitalism.

One of the difficulties in the book is its polemical edge. Though it objects to the Althusserians' mistake for treating Marx's historical categories as a priori constructs, Sayer nowhere presents a full-scale analysis of Althusser's "mistake." Since Sayer's understanding of Marx's method involves going "beyond" Marx's own analysis and method, it is worth commenting on where this going "beyond" leads to. In chapter 5, the author draws on Kant's *Critique of Pure Reason and Prolegomena* to establish a methodological parallel between Kant and Marx. We are informed that what the "faculty of reason" means to Kant "economic category" means to Marx (p. 105). This parallel of Kantian and Marxian critique is predicated on suspending "Engels' testament" of Marxist thought. In so many words, if we excise Engels' categorization of Marxism as "scientific" then we can establish the methodological analogy between Kant and Marx.

Sayer's attempt to see parallel between Kant's "transcendental analytic" and Marx's "dialectics" merits two comments. First, by going "beyond" Marx, Sayer should have encountered the writings of Georg Lukács (1885–1971) who showed (cf. *History and Class Consciousness*) that there can hardly be a parallel between the Marxist method and that of Kant. Lukács had ascribed a tragic quality to Kant's philosophy because it left unresolved the duality of man's being. Second, even though both Kant and Marx shared an attempt to overcome the contingency of human existence, Kant's attempt involves the setting up of freedom and reason as the ultimate goal of man's perennial pilgrimage toward himself. The Marxist concept of freedom and reason are inhospitable to Kantian ideas.

Somewhat inconsistent with the theme of the book, Sayer sees Marx as a "scientist" and his *Capital* as the "paradigm of scientific research" (p. 141). To insist on Marx as a "scientist" belongs to the baroque period of Marxism. In the closing decades of this century this can be done if Engels' testament is left intact. But Sayer himself suggests the removal of Engels in order to establish the Kantian-Marxist methodological parallel. One wonders what *explanans* and *explananda* there is for Marx the "scientist" and Marx the Kantian.

ARPAD KADARKAY

University of Puget Sound

Nationalism in the Twentieth Century. By Anthony D. S. Smith. (New York: New York University Press, 1979. Pp. x + 257. $17.50, cloth; $8.95, paper.)

Assume that we are able to ascertain that the people of state A are highly nationalistic and the people of state B are not. Does this information allow us to predict differing behavioral tendencies in the two polities; and, if so, what are those tendencies? If the case is made that the United States and China are examples of state A and the Soviet Union and India of State B, anyone could see that the question is not trivial. It is also glaringly obvious. But the fact is that social scientists concerned with nationalism almost never ask it. Anthony D. Smith is no exception to this rule in the body of his study. But in his conclusions he does give a most surprising answer.

The questions Smith and other authorities do ask concern the historical rhythm of nationalist movements—when and why did nationalism appear; which elements of the population are most involved with it at various stages; what is its future? In writing this book and his earlier *Theories of Nationalism*, Smith has joined the vanguard of authorities on nationalism. His erudition is most impressive. There seem to be few ethnic elements that have escaped his attention. And his analysis is always insightful, provocative and solidly grounded in historical references. His primary foci in *Nationalism in the Twentieth Century* are on the relationship of nationalism with fascism and Marxism and on what he sees as a resurgence of nationalism in the West and its growth in other parts of the world. These are central concerns for the student of nationalism and Smith's theses deserve the attention of the field.

Most social scientists would no doubt agree that "nationalism" is one of the most important determinants of contemporary political behavior. But the failure to address the question of what kind of behavior it induces is often a product of a previous failure of conceptual construction. There is no self-evident definition of nationalism and different authorities choose to embrace quite different elements in their definitions. A failure to advance and, more important, to remain true to an operational definition of the concept is likely to lead to inconsistency and a tendency to reify. Smith is seriously guilty on both counts. Nationalism lives and breathes in this book. Most frequently the organism is liberal. "It aims to build a nation, to construct a world of nations, each free and self governing, each unique and cohesive, each able to contribute something special to plural humanity" (p. 29). Mazzini's dream has come to life! Smith tells us nationalism is an ideology but its components are never consistently spelled out. We learn that nationalism is able to grasp "the importance and profundity of mass religious sentiments" (p. 31); that it has "aimed to give concrete political form to the aspirations of both rationalism and romanticism" (p. 82). But these insights are in the form of undeveloped assertions.

Smith does come tantalizingly close to conceptual construction. At times he treats nationalism in belief system terms (p. 116) and argues that it can "combine with other ideologies" (p. 56) or belief systems. But he does not follow through and, for example, develop pictures of different belief system compounds which share a nationalist component. In his analysis of the critically important relationship of nationalism and fascism, he could easily have dealt with nationalism as an important component of an umbrella belief system we call fascism. He does at one point worry about a possible nationalism-fascism combination (p. 179), but by and large he treats them as competing ideologies, the features of which he boldly asserts.

What is the impact on nationalism when attachments to other identity communities, in particular race, religion and ethnic communities, complement or compete with an attachment to the nation? Few questions are more obvious and more perplexing to the student of nationalism. But Smith avoids grounding his analysis in any identity theory. He treats at length the interaction of nationalism and racism but very little in terms of identity and complementarity. What he does is to construct a scale for classification purposes (p. 99) that goes from nationalism through racial nationalism and nationalist racism to racism. The indicators of scalar differences are left implicit but appear to be more a factor of liberalism than identity.

More seriously damaging to his book is the treatment of the relationship of nationalism to ethnicity. For the most part he treats loyalty to an ethnic group as a central aspect of the definition of nationalism. But is there such a thing as a multi-ethnic nation, the United States, for example? If so, what tensions are likely to be manifest? He avoids this question even though much of the last part of the book is concerned with the phenomenon of ethnic nationalism in multi-ethnic Western states such as Canada, Belgium, and the United Kingdom. These manifestations indeed constitute much of his evidence for a contention of nationalist resurgence. But since he is not inclined to look closely at inter-ethnic relations within a national context, he pays little attention to the familiar relative deprivation explanation for this phenomenon. He makes a more general point and it is a major one in his book: there is in this nationalist resurgence a rebellion against the technological and bureaucratic state and a demand for self-liberation. The thesis is interesting, but since it is more asserted than developed, it is not persuasive.

Most surprising in the book is the omission of any real comparison of the multi-national state and the nation-state. But in his final pages, Smith really explains why he would not see this as an important omission. He tells the reader (p. 195) that he believes that if there are unions of many existing states into great superstates, the resulting polity will behave very much like nation-states. In other words, there are *no* behavioral tendencies associated with nationalism on the state level. One wonders then—if nationalism is epiphenomenal—why Smith bothers to write books on the subject.

RICHARD W. COTTAM

University of Pittsburgh

Theory of International Politics. By Kenneth N. Waltz. (Reading, Mass.: Addison-Wesley Publishing Co., 1979. Pp. iv + 251. $7.95, paper.)

Theory of International Politics is really two books in one. The first subjects well-known theories, primarily propositions about the balance of power and about imperialism, to tests derived from the philosophy of science. In the second study Kenneth Waltz does his own theorizing. The critique is evidently a ground-clearing operation, and Waltz finds it appropriate to publish it and his own theory in a single work.

Most of the critique appeared in Waltz's 1975 essay in the *Handbook of Political Science* and was favorably reviewed by Stanley Hoffmann in this journal. A first extensive rejoinder has come from Morton A. Kaplan in his 1979 collection of essays, *Towards Professionalism in International Theory* (Free Press, 1979). Kaplan devotes 90 pages to discussing "Waltz on Kaplan." "Kaplan on Waltz on Kaplan" puts the ball back in Waltz's court. I await with bated breath "Waltz on Kaplan on Waltz on Kaplan." These two formidable contributors to international theory do agree on one point: each proclaims the present barrenness of international theory. It is ironic that two exemplars of its fertility should reach identical judgments in works which testify to the contrary.

As critic, Waltz defines his task in ways that, though not unreasonable, heighten the impression of overall failure in international theory. His rigorous philosophy of science tests of the theories he does consider yield results that are mostly negative, and his sharp focus on the structure of a system dominated by a few great states precludes any consideration of exciting speculation outside that focus.

"Theory" in my view is any form of disciplined generalization with apparent explanatory power. A new Plato or a new Aristotle writing in the field of international relations would under this definition be a theorist whether or not he plays the theory game according to Waltz's philosophy of science rules. That game is well worth playing; but someone playing the theory game according to other than Waltz rules, e.g., Robert Merton's, as described in his *Social Theory and Social Structure* (Free Press, 1968), does not thereby necessarily provide one more example of general failure among international theorists.

Failure in theory takes two forms, the total failure of a start in the wrong direction and partial failure of not going the whole way down Stanley Hoffmann's "long road to theory." The distinction between total and partial failure is essential if the critic is to leave room for theory to cumulate. Kenneth Waltz writes of J. A. Hobson's "fatal difficulty" (p. 24), that Hans Morgenthau and Graham Allison "have confused and merged two quite different matters" (p. 122), that Johan Galtung's descriptions "fail to correspond with reality" (p. 38), and lead him to unwarranted conclusions (p. 32), that Immanuel Wallerstein "confuses theory with reality" (p. 38), and that A. F. K. Organiski's assumptions are "loosely stated" (p. 119). In evaluating Stanley Hoffmann's work, Waltz writes that "the carelessness with which systemic inferences are drawn is matched by the vagueness with which the system is defined" (p. 48). Whether or not some (or all) of these judgments are warranted, the tone suggests that in the theory game we must all go back to Square One.

Is it *a* theory of international politics or, on the "back to Square One" assumption, the foundation and framework of *the* theory that Waltz has built in the second part of his book? The ambiguity of the title, *Theory of International Politics*, might not have been noticed were it not for Waltz's view that "nothing seems to cumulate, not even criticism" (p. 18). There is need for theory about a multi-state system in which only the very few great states count, and those few are posited to be very much alike in ways that relate to that theory. After all, this is the model world of Raymond Aron, of Hans Morgenthau, of most strategic theorists, and of several of Kaplan's six systems. As *a* theory, Waltz's theory of international politics takes its place in a distinguished group, especially because of its provocative emphasis on the potential for stability in a two-power system.

States are not, however, the only actors in the world system, great states are not the only nation-state actors of interest to theorists, and the theorist need not always posit that the great states are alike. Theory about world politics must deal with more than world political oligopoly. It must deal with world mass and with relations between oligarchs and all the rest. It must specify the circumstances under which the cast of characters, great and small, changes and how states ascend to the first rank and descend from it. And it must deal with Hedley Bull's "new mediaevalism" and the Sprouts' "politics of the Planet Earth." In this comprehensive theory, there would be a place for the seminal contributions of Karl Deutsch and Harold Lasswell, only one of whom Waltz even mentions.

It is no criticism of *Theory of International Politics* to say that a Waltz theory of inter-great power politics is not *the* theory of world politics, especially because its author makes no such claim for it. In the dialectic of theory development and the slow process of theory cumulation, the Waltz book will not be ignored.

WILLIAM T. R. FOX

Columbia University

American Politics

A History of American City Government: The Formation of Traditions, 1775–1870. By Charles R. Adrian and Ernest S. Griffith. (New York: Praeger Publishers, 1976. Pp. v + 233. $10.00.)

This volume is the most recent in a projected series of five designed to cover developments in American city governments from colonial times to about 1940. Ernest Griffith published the first volume, *The Colonial Years,* in 1938, and followed that with two more, *The Conspicuous Failure, 1870–1900,* and *The Progressive Years and Their Aftermath, 1900–1920,* both appearing in 1974. The present book fills the chronological gap in Griffith's work, starting with the post-revolutionary transition from colonial forms of local administration and moving on through years heavily influenced by Jacksonian democratic developments and westward expansion. Charles Adrian joins Griffith as coauthor and, according to the preface, intends to carry the series further with a fifth volume covering the years from 1920 to 1940.

The organization of *The Formation of Traditions* differs from the previous volumes; it is arranged by topic, e.g., "The Legal Status of Cities," "Intergovernmental Relations," "Municipal Finance," etc., rather than chronology. Within chapters, general chronological developments are traced through references to changes and illuminated with some wonderfully apt quotations and anecdotes. In the chapter on "The Suffrage, Candidates, and Campaigns," one learns that the 1776 New Jersey constitution extended the right to vote to all who were 21 and owned 50 pounds worth of property, categories including women, as the authors note, "apparently by oversight" (p. 113). But, sexism regained the field following a wildly corrupt referendum election in 1806 called to decide the location of new county courthouse for Essex County. Newark and Elizabeth vied for the prize, Newark managing to tally a highly suspect 5,039 votes in a township whose total population was under 6,000 (p. 116). The election reforms which followed limited the franchise to males, but eliminated the property qualification (p. 113).

In a note sent with the review copy, the publisher wrote that copies were inadvertently distributed late (1979) because of a transfer of records from Praeger to Holt, Rinehart, and Winston. That delay explains the lateness of this review and its abbreviated form. Still, urbanists can benefit from a reading of this history, if only to learn that much that seems new in city politics isn't. New Orleans experimented with ethnically homogeneous neighborhood governments in the first years of the nineteenth century (p. 122), single-issue groups wreaked havoc on settled electoral and political patterns (p. 125), and municipal defaults on debts gave rise to "innovative" palliatives in the form of lotteries (p. 205). The book has its weaknesses, the main ones being a lack of footnotes and a decidedly "survey" character; but I will be eternally grateful to Adrian and Griffith for telling me that New Orleans' 1805 charter gave the mayor authority to appoint "the weighers, constables, scavengers, and wharfingers" (p. 188). I plan all sorts of lecture uses for that tidbit.

RAYMOND E. OWEN

University of Pittsburgh

Congress and the Bureaucracy: A Theory of Influence. By R. Douglas Arnold. (New Haven, Conn.: Yale University Press, 1979. Pp. xiii + 235. $17.50.)

"The central objective of this book is to determine the extent to which American congressmen influence bureaucratic decisions concerning the geographic allocation of expenditures" (p. 3). To accomplish this goal, R. Douglas Arnold first evaluates existing theories of influence, then develops his own set of expectations about geographic allocations, and finally examines what happens in regard to military employment, water and sewer grants, and model cities grants.

In the early chapters, Arnold systematically reviews the theoretical and empirical work that might be useful to him and sets up his own approach (both theory and measurement). In chapter 1 he reviews three theories of decision making that might apply to geographic allocation and finds them wanting. He then proceeds in the next three chapters to build a rational theory of how bureaucrats are likely to make allocations—first by examining congressional and bureaucratic political goals, then by discussing the strategies for building coalitions of support. The theory in a nutshell is that "bureaucrats . . . allocate benefits strategically [among congressional districts] in an effort both to maintain and to expand their supporting coalitions" (p. 207). These chapters are well written and professionally developed but simply too long. It all could have happened in about one-third to one-half the space.

The empirical part of the book begins with chapter 6. The pattern is the same for each of the chapters. Arnold first establishes reasonable explanations for bureaucrats' allocational decisions, bearing in mind the political goals discussed above (theirs and those of members of Congress). He then tests these explanations with data on the actual allocations—e.g., closing bases, site selection, increasing employment for the military, and approval of grant applications for the water treatment and model cities programs—employing bivariate and multivariate analysis in each case. Arnold discovers that bureaucrats apparently pay attention to the committees with jurisdiction for their programs, try to reward their supporters and selectively entice their opponents, and tend to maximize their limited resources. He also finds that the precise allocational strategies vary with the program.

The surprise is not in what Arnold finds. It is rather in the realization that no one has quite done it in this way before. The book's contribution, then, lies in its focus, perspective, and methods. It should stimulate further research on the many policy roles of the Congress. And that's saying a lot for a scholar's first book.

CHARLES O. JONES

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Race for the Presidency: The Media and the Nominating Process. Edited by James David Barber. (New Jersey: Prentice-Hall, 1978. Pp. vii + 205. $10.95, cloth; $4.95, paper.)

Race for the Presidency is a significant book. Its significance, however, lies more in what the study attempted to do than in how well it was done. The authors are concerned with the role of the media—primarily television—in the nomination phase of the electoral process. Their observations are drawn from the 1976 primary election period.

This is a welcome departure from traditional media studies that have focused on electoral behavior in the narrow time band immediately before and after national elections. As citizens have often formed evaluation of the parties, candidates and dynamic issues of the election prior to this time it is little wonder reinforcement effects appear so prominently in the literature. The temporal issue has no doubt contributed to the disparity between the "conventional wisdom" that TV has dramatically changed the conduct of American politics and empirical studies that chronicle a legacy of non- (or reinforcement) effects. To understand the role of television more fully, one needs to study the electorate at an earlier point in time. *Race for the Presidency* represents a step forward in this regard.

But the studies are quite disjointed. Little new ground is broken individually, and the collective project offers little in the way of new directions. The authors are concerned with describing the role of the media in nomination politics from a variety of perspectives, "groping," as Barber puts it "for the journalist–politician nexus" (p. 1). Paradoxically, the differing perspectives are both a strength and weakness of the volume. While the variety broadens the explanations of events, they are poorly integrated. Each author comes to a set of findings influenced by the perspective of the group focused on (the campaign organization, individual reporters or the broadcast networks). The different vantage points occasionally yield different explanations for the same phenomenon. Yet the differences are nowhere indexed, much less discussed. An integrative chapter or summation after the first four studies would have yielded interesting questions of discussion and perhaps a framework for further analysis.

While the book does not hang together well, many of the studies are interesting. F. Christopher Arterton does an insightful job of examining the candidate-journalist nexus from the perspective of senior campaign staffers. He notes how the "media event" mentality has become pervasive, as campaign managers take the probable media interpretation and style of coverage into account. The journalistic community is perceived by campaign managers as an alternate electorate. "Accordingly, the attitudes, beliefs and behavior of the journalistic corps becomes a milieu for political competition between presidential campaigns" (p. 12). In the first two chapters of the book Arterton does a sensitive job of discussing and illustrating the dynamics of, and bargaining in, candidate-journalist relationships.

Donald Matthews' chapter is concerned with the "winnowing" or filtering process by which the field of candidates is pared down. Little new ground is broken in his discussion of the media's preoccupation with the "horse race" aspect of electoral competition, overemphasis of the first primary (New Hampshire), and minimal concern with issues. Matthews argues that the media's greatest opportunity to affect outcomes comes when the audience is paying the least attention—early in the primary season. The lack of few clear cues gives the journalistic community (almost by default) latitude in setting expectations against which the candidates actual performance may be judged. After discussing some of the criteria used by journalists to arrive at this "consensus of expectations," Matthews turns his attention to sorting out the conditions under which issues become "news" items. His assertion that a change or inconsistency in a candidate's position, or an impractical position, makes news leads to a revealing look at the mind-set of individual journalists and the news attributes valued by news organizations.

While offering some gems, Matthews also makes some claims that need better support. While evidence for some of the explanations he ventures is hard to gather, one would have liked to see more alternative explanations raised. Matthews asserts, for example, that "the news media have helped create an office whose incumbent is virtually unbeatable in nominating contests" (p. 77). As evidence, he cites Ford's defeat of Reagan, who was "the strongest challenger to an incumbent President in more than half a century" (p. 77). One could as easily use this "evidence" to support the counter-claim that without the news behavior described, the Reagan challenge would not have been as strong as it was.

William Bicker's contribution looks at the 1976 election from the network's side of the camera. His research questions include how the networks planned to cover the primaries, how they implemented their plans, and how successful they were.

Network executives reacted to criticisms of their 1972 coverage by planning to shy away from "defining front-runners," focusing less on the "horse race" aspect, and by devoting more attention to "issues." These "reforms" were to be accomplished by defining a primary winner by who received the most votes rather than surpassing expectations, going into as many states as possible, and making extensive use of in-house polling. The reforms met with mixed success.

Bicker appears to credit the networks with more success in sticking to the "votes" criteria than Matthews—perhaps a result of their different vantage points. The networks were less successful in covering issues—saying that they could not find any. Bicker attributes this to the low production value in issue discussions or "talking heads" on television, among other reasons. The organizational definition of news (conflict, drama, personalities, color) militates against coverage of issues. While Bicker and Matthews make the same observations, Bicker works from a general acceptance of the constraints perceived by the network organizations and is far less critical than Matthews.

James David Barber's three chapters focus on the journalist in different roles. News reporting is first a literary endeavor requiring the journalist to develop a "story line" or theme that can be developed over the course of the campaign. This leads journalists "to ignore commonalities among the candidates, in favor of the distinctive" (p. 121).

Barber also looks at the journalist as a psychologist and educator. In the former role the journalist is charged with giving us "a perspective on the candidates as potential Presidents" (p. 145). This is also part of the education function reporters and television news could fulfill. The education roles have passed to journalists almost by default, as a large part of the citizenry is solely dependent on television for political information. Barber argues for more journalistic license in both roles encouraging reporters to try saying, "flat out, what Democratic goods and bads are to this and that happening" (p. 191).

It is easy to get absorbed in Barber's literary analogies and flowery prose and by-pass some central questions that are not addressed. First, there is an assumption that journalists can find "the truth," when in fact there are many competing truths to any story. Moreover, there is a great deal of implicit trust in journalists' ability to make psychological judgments about candidates. This faith may be fully warranted in the Broders and Apples, but the campaign press corps is not comprised of Broders and Apples. It may well be that more journalistic latitude will produce "better" coverage, but the minuses also need to be considered in the equation before a balance may be found.

CLIFF ZUKIN

Rutgers University

The Office of Management and Budget and the Presidency, 1921–1979. By Larry Berman. (Princeton, N.J.: Princeton University Press, 1979. Pp. xiii + 180. $15.00, cloth; $4.95, paper.)

The relative scarcity of research on budgeting institutions and decisions in the national executive branch remains a serious gap in the literature of political science. Berman's new book is "institutional" in focus and describes the evolution of the staff roles served by the BoB/OMB since 1939 (the pre-1939 period receives scant attention). Although his account adds a useful historical dimension to other recent discussions of this topic, Berman's basic themes are already familiar—the politicization of the OMB and the historical efforts to improve its inadequate administrative management function. The familiarity of the themes would not be a flaw of the book if there were not so little discussion of their implications for the future of the OMB. A number of important questions are not even raised, much less discussed. There is also a conspicuous absence of any discussion of the traditional budgetary function of the BoB/OMB and how it influenced or was influenced by these other changes in the bureau.

Berman's major theme is the inherent conflict between the institutional staff role the bureau played in its earlier days, the more personal/political staff roles (Berman seems to equate these two) which it was called upon to perform more and more frequently, and the dilemmas this has posed for its performance, status, and influence as a presidential staff agency. As the bureau's institutional staff role of provider of objective and non-political analysis and intelligence to the presidency led the BoB to its height of influence, the (apparently) irresistible demands made on the bureau for personal and political assistance also increased. The politicization which reached an extreme in the Nixon administration had really started back in the Truman years and was well advanced by the mid-1960s.

A minor and overlapping theme of the book is the historical inadequacy of the bureau's administrative management support function. The bureau's periodic reorganization efforts have almost always been directed at improving this staff function. In a way this has further contributed to its politicization because it was the bureau's managerial function that involved it in the operational problems of expanding government programs and into the political thicket of intergovernmental coordination.

Although Berman is not much concerned with the traditional budgeting function of the OMB, it does not seem too much to expect a treatise on the OMB to deal with the way the budgeting function, particularly the major reform efforts, was affected by changes in staff roles. Yet a fairly lengthy discussion of the BoB involvement in program development and coordination during the Johnson years gives no indication of how PPBS fit into the picture. There is only a single passing reference to the MbO initiative.

Berman leaves us the task of speculating as to the causes and possible consequences of OMB politicization—e.g., is the decline of "neutral competence" due to structural or personal factors? Could the OMB have avoided politicization by stressing traditional roles? Can the integrity of the OMB's institutional function be restored? Might the OMB's status be dependent on whether the economic mood is one of economizing or expanding? The lack of attention to these issues is, in my view, the major shortcoming of the book.

JOHN R. GIST

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Force without War: U.S. Armed Forces as a Political Instrument. By Barry M. Blechman and Stephen S. Kaplan et al. (Washington, D.C.: The Brookings Institution, 1978. Pp. xviii + 584. $19.95, cloth; $8.95, paper.)

Barry Blechman and Stephen Kaplan are concerned with the use of armed forces by the United States for political purposes. Noting that the mere possession of such forces can have a variety of political effects, they define their interest more precisely as involving the use of particular forces for particular objectives in particular situations; specifically, physical action by uniformed services in deliberate international influence attempts that fall short of continuing contests of violence (p. 12). They aim both to describe the 1946–75 historical record of such events, attending to such contextual factors as the size, type operations, and situation of the forces involved, as well as to assess the effectiveness of such exercises. These goals are addressed primarily by simple quantitative analyses of nominally and ordinally scored variables, which provide the basis for broad generalizations about relationships and trends. The authors are also concerned with achieving a micro-level understanding of the factors involved in these operations, and to this end they supplement the aggregate data with ten fairly detailed case studies, prepared by area experts.

The latter component of this study is by far the more readable and probably the more generally useful. Five authors compare two incidents apiece in terms of the political utility of U.S. armed force, addressing specifically the nature of U.S. objectives and policy instruments, the characteristics of the situations, and outcomes. As a group, the 10 case studies are well chosen to serve as a representative sample of the overall data set, and the paired comparisons are well selected to illuminate consistences and differences in apparently similar contexts. All of the studies seem generally well done and stimulate a wealth of research propositions; most address issues that transcend the specific event being considered. David Hall's analysis of the 1962 Laotian war and the 1971 Indo-Pakistani war is particularly notable for its systematic, careful comparisons and research-provoking conclusions. The other comparisons include contrasting the 1958 Lebanese intervention with the 1970 Jordanian crisis; the 1961 ouster of the Trujillos with the 1965 Dominican intervention; the Berlin crisis of 1958–59 with that of 1961; and the 1951 Yugoslavian crisis with the 1968 Czechoslovakian intervention.

The aggregate data component of this study consists of information collected about 215 incidents that occurred from January 1, 1946 to December 31, 1975. A proportionally structured sample of 33 cases (15 percent) was used to investigate questions about the utility of armed forces for political objectives, and the bulk of this section depends on correlational analysis of objectives and outcomes in this sample. Blechman and Kaplan are too apologetic about the size of this sample and the simplicity of the statistical tools they employ; there are sound reasons for proceeding as they did, and the ground is well prepared for more refined analyses in the future.

Nonetheless, the aggregate analysis is much less satisfactory than the case studies. Its failing is not with statistical technique, however, but rather with the absence of a systematic presentation of research design. The reader can often infer the nature of the variables that were coded and the scoring scheme for them, but a summary codebook would prove helpful. Even better would be a discussion of the logic underlying the selection of variables, together with a conceptual and empirical justification for the coding scheme. These additions would very likely help to improve the presentation of research findings, which at present are reported essentially on a variable-specific basis. The intrinsic interest of these frequency reports varies, and in general it is left to the reader to invest them with significance. The data have not been organized or applied to improve our understanding of the political use of force; in this respect, Blechman and Kaplan have not pursued research problems but have reported some statistics. One's sense of disappointment in this approach is sharpened because the relevant analytic literature seems quite ready for an empirical approach along the lines of this study. But the literature is not well integrated even in the limited conceptual discussions that do occur; for example, some space is devoted in both chapters 4 and 11 to the relative ease of reinforcement actions as compared with modification actions, but no reference is made to the standard Schelling or Ellsberg formulations of the same issues.

The concluding chapter presents an attempted synthesis of the two approaches. This effort takes the form of illustrating certain aspects of the aggregate analysis with details from the case studies, and by and large this approach is carried out well. The chapter is not, however, completely successful. In part, the integrated effort is hindered by the limitations of the approach taken to the aggregate analysis, but in part it also seems that the case studies were not fully digested. Blechman and Kaplan, for example, read Hall's analysis as showing that "U.S. support for Pakistan during the 1971 war with India was a relatively empty gesture; . . . deployment of the *Enterprise* task force to the Indian Ocean had almost no effect on the decisions of either . . . India and Pakistan [or] . . . China and the Soviet Union" (p. 518). In fact, Hall's analysis suggests that the U.S. military actions had quite an effect, albeit contradictory to U.S. objectives; for example, "The net effect of Task Force 74 was to drive India closer to the USSR, arouse anti-American passions, and prompt effective military countermeasures" (p. 215), as well as to convey "the illusion of Soviet deterrence of American intervention" and to give Pakistan "the misleading impression of imminent allied intercession . . . [and thus] to postpone by several days Islamabad's acceptance of a cease-fire" (p. 216).

Overall, *Force without War* is to be recommended more for the contributions it could make to research, through further exploitation of the case studies and aggregate data, than for the contributions it embodies.

ROBERT LYLE BUTTERWORTH

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Franklin D. Roosevelt and American Foreign Policy, 1932–1945. By Robert Dallek. (New York: Oxford University Press, 1979. Pp. xii + 657. $19.95.)

This may or may not be the best book on Franklin Roosevelt; I certainly haven't read them all. It is important because of its use of new materials from archives (maybe most interestingly, minutes of British Cabinet meetings) and of extensive public opinion survey material. It will also be one of the most satisfactory studies for political scientists, as it is a lively and deeply informed account of Roosevelt's foreign policy acts as imbedded in his perceptions of what was feasible under domestic and external constraints. Roosevelt emerges as, in Dallek's terms, an "idealist as realist." He had a clear view of what he wanted to achieve, and a sharp perception of the limits imposed by domestic political structures. One can begin to appreciate the skills that allowed him to be elected president four times—a feat that seems especially remarkable in this era.

We see a man with a markedly "internationalist" vision, stemming from extended annual visits to Europe as a child, and a readiness for international military involvement to "allow him to live out his fantasy about literally following in T.R.'s footsteps" (p. 9). He advocated a strong defense before and after World War I, but by 1935 the sort of vision he largely shared with "bankers and businessmen concerned with debts, currency, and trade, and idealists principally urging an American contribution to peace" (p. 97) was dominated by the country's isolationist mood.

He was a "realist" quite capable, in 1933, of limited interference against a "radical" regime in Cuba. During World War II he held no illusions about the stability or power of Chiang Kai-shek, or about that of China for the near future. But he did envision China as a major power some 20 years in the future, and did his best to build up China as a potential American ally and counterweight to Japan and Russia. Always the conscious architect of a postwar world order, he engaged in no wishful thinking about his ability to get along with "Uncle Joe" Stalin. In dealing with Stalin he felt bound by the limits of American power. After intensive effort Roosevelt recognized that he could not prevent Stalin from imposing a communist government on Poland, so he tried (unsuccessfully) to get him to stay his hand until the American 1944 presidential election was past. His plans for postwar American participation in the occupation of Germany were carefully balanced between a kind of occupation that he thought would be supported by continuing American opinion, and a wish to extend the permissive bounds of that opinion by putting the troops in the right places. His conduct was always determined not only by what he thought militarily desirable and feasible, but by his felt need to build a unity in the United States that would keep the country in the war and involved overseas during the peace.

Roosevelt has been widely recognized as a master of bureaucratic politics, pitting one agency or advisor against another so as to preserve his own freedom of action. Even so, Dallek gives us vivid examples where bureaucratic politics thwarted the president's aims on important matters, such as by sabotaging aid to Russia in 1941 and by converting his desire for a partial oil embargo of Japan into a full-scale one. His sensitivity to public opinion—marked by close attention to public opinion surveys, often as brought to him by his adviser Hadley Cantril—is a continuing focus of the book. In his 1940 and 1941 policies Dallek maintains that Roosevelt did not lag behind public opinion; if anything he was "abreast and possibly a little ahead of public feeling" (p. 227) in his desire to involve the United States in the war and if necessary to create an incident "which would justify him in opening hostilities" (p. 285).

While Dallek substantially approves of Roosevelt's prewar and wartime policies, he does not ignore their unattractive features. He admits that the dilemmas of Jewish refugees were of little interest to FDR; he "allowed domestic and international constraints to limit him to a series of small gestures" (p. 168). In the matter of interning Japanese-Americans during the war, Roosevelt again failed to take a stand on principle against prevailing opinion. Dallek harshly declares, "In these matters, there is considerable truth in Clare Booth Luce's observation that 'every great leader had his typical gesture—Hitler the upraised arm, Churchill the V sign. Roosevelt? She wet her index finger and held it up' " (p. 336). Dallek also articulates his distress about Roosevelt's illegal use of the FBI to open mail and otherwise illicitly investigate the acts and attitudes of Americans, and his manipulation of public opinion with false information about the naval engagements in the North Atlantic in 1941—engagements which he blamed on the Germans though they in fact were initiated by American naval forces. Dallek sees at least the manipulation of public opinion as necessitated by Roosevelt's laudable aims and the isolationists' opposition. Though it did create a precedent for later, more questionable cases, "it was in the country's best interest" (p. 290). As James Madison said, "War is . . . the true nurse of executive aggrandizement."

Dallek is at his best in assaying Roosevelt's delicate maneuvering within the bounds permitted by public opinion, and in his own assessment of American politics as it did limit Roosevelt. I find Dallek's parallel assessments of the limits imposed by external constraints to be less carefully reasoned. For example, he states (agreeing with my opinion expressed elsewhere) that Roosevelt "greatly exaggerated the German threat to the Americas in the summer of 1940" (p. 235). But more arguably, "Whatever the actuality of Hitler's plans or future actions, a rational leader had to view a Nazi drive against the hemisphere as a possibility against which he must plan" (p. 236). This represents the core both of Dallek's own view and of Roosevelt's behavior: he was highly risk-averse; he often devoted his energies to avoiding the worst outcome. This caution helps us understand how he could indeed have been elected president four times. It also is a pattern we saw again in another (for a while) extraordinarily successful political leader, Lyndon Johnson, who in trying to avoid the worst case certainly fell into a very bad outcome. Roosevelt was a great political figure; probably even a great president. But, as in all human affairs, "There's no free lunch."

BRUCE RUSSETT

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Policymaking for Social Security. By Martha Derthick. (Washington, D.C.: The Brookings Institution, 1979. Pp. xiv + 446. $11.95, cloth; $4.95, paper.)

This book is a case study on U.S. social security policy, emphasizing the specialness of this policy sphere more or less to the exclusion of broad generalizations of the contextual or comparative sort. The description in detail is, however, mitigated by the analytic organization of material, clarity of style and felicity of phrase. For many this book may present more about social security than they ever wanted to know—but its very accessible use as a source book is important to keep in mind.

It seems there have always been many friends of social security who saw it as epitomizing, in ideal form, the just, efficient, and dedicated agency. To these buffs this book should be as welcome as manna from heaven. They will find it all there: the beginning; the founders; supporters; opponents; the conflicts and coalitions; and the present dilemmas. Good reading all, for those so motivated.

For a larger group of students of public policy there may be more delving than outright consumption of this text. For these, Derthick's conceptual labeling should be most helpful. The first part on "participants" deals with those interested positions and groups claiming a significant role in social security policy, including opposition. Also public opinion (in part, parties and mass movements) elements are examined.

In a second part substantive issues are treated extensively, in a manner quite descriptive of what the actors have articulated. Those analytic issue categories developed by political economists, Lowi, or the theorists of justice, redistribution, relative deprivation, receive only passing mention.

One has to interpolate here. Adequacy (related to contributions) is seen as gaining over equity, as changes take place in the social security program. Symbolic politics were continuously resorted to, in the call for pure principles guiding choice, but most often this was in the actual context of incremental changes, and a pragmatic orientation on the part of the key program executives. With benefits being continually expanded, costs of the program became a prime concern. But seldom was the cost issue raised clearly. Instead, the trust fund was symbolically characterized as a guarantee to future generations; taxation rate rises were said to cover current costs and thus claimed to be "fiscally responsible."

As Derthick points out very well, ultimate redistribution in cost-benefit terms does not lend itself to easy issue-making in this case. This is because the pattern of transfer runs across "statistical categories" rather than broadly organized groups or strata; from active workers to elderly nonworkers; from single workers, to married workers; from long-time contributors to late entrants. There are differential benefits to widows versus widowers, female versus male dependent survivors, working women versus nonworking women. (In the latter cases, as Derthick points out, the women's movement has become activated on these issues.) In spite of all these differences, there were sufficient political resources for key program supporters to mitigate actual conflict.

In a third part Derthick chooses three cases where significant social security policy choices were made: the assumption of disability coverage; the adoption of Medicare; and substantial increases in cash benefits. She examines the political circumstances, especially the extent of conflict over these major changes.

Overall, what Derthick develops seems to be a politics of insiders. Throughout she has noted the broad initiatives and means of manipulation and adaptation available to the "program executives," by which to avert conflict or losses. When it is all boiled down I see three whom this characterizes most: Arthur Altmeyer, Robert Ball, and Wilbur Cohen. On the legislative side, Wilbur Mills comes closest to exercising major discretion, though as Derthick notes, the committee politics is also coalition and consensus politics. This seems to me to suggest a model of bureaucratic elitism in what was acknowledged to be an autonomous policy sphere. Yet the author dismisses elitism as a school of thought not given much attention by policy scholars!

She would like to see more "politics" in this policy sphere, namely conflict and agitation. Yet her own accounts seemed to show active partisan involvement as well as legislative executive and intra-executive cross-currents. It seems to me therefore that the fault lies with the power of the program executives.

This book provides much food for thought, especially in consciously working over theory. Without such work, the book might seem to present merely a unique case. With it, we can learn from it and compare U.S. social security policy to other such policies here and abroad. Usefulness in the comparative sense is the best ultimate test that the case has contributed a meaningful set of political patterns.

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The Reform of FBI Intelligence Operations. By John T. Elliff. (Princeton, N.J.: Princeton University Press, 1979. Pp. xi + 248. $14.50.)

John Elliff is the nation's leading authority on the operations of the Federal Bureau of Investigation as these touch on politically active groups and individuals. As chairman of the Domestic Task Force of the Church Committee staff, Elliff reviewed hundreds of thousands of pages of the history of the Bureau's domestic intelligence operations, and the portion of the committee's report dealing with this subject is the definitive history of that aspect of the Bureau's performance.

From the Church Committee, after a brief interlude with the Police Foundation in Washington during which the chapters of this book were initially drafted, Elliff was hired by Senator Birch Bayh as a specialist in intelligence matters, and is Bayh's principal "staff person" in his capacity as chairman of the Senate Intelligence Committee. In this job Elliff has stayed in close touch with the problems of FBI reform even though that issue is, for legislative purposes, before the Senate Judiciary Committee. With a proposed investigative charter for the FBI now before Congress, it is particularly useful to have this uniquely qualified participant-observer's thoughts on the future of FBI intelligence operations summed up in a single volume.

In the atmosphere of emotionalism and overstatement generated by the intelligence scandals of the mid-1970s, Elliff's policy recommendations come as a welcome breeze of moderation and good sense. While recognizing the extreme hazards posed to individual privacy and free political association by the FBI's involvement with politically active groups and individuals, Elliff is not swept into simple-mindedness. Convinced that some FBI intelligence operations are necessary to effective law enforcement, Elliff goes on to suggest how these can be carried out without constitutional violations or unacceptable intrusions into privacy.

The organization of the book is quite straightforward. The first two chapters review the controversy over FBI intelligence operations, and briefly summarize the various positions on the issues outlined—all the way from close such business down completely to continue business as usual. Chapters 3 and 4 ("Development of the Attorney General's Guidelines" and "Reorganization of the FBI") present in a very sensitive and balanced way the reforms that have already been undertaken as a result of Attorney General action and internal Bureau action. These are followed by two chapters, the best of the book, discussing the particular problems involved in domestic security, intelligence operations directed at preventing terrorism, and the difficulties of our counter-intelligence operations. In the final chapter, titled "Professional Responsibility," Elliff delivers himself of his principal observations concerning the future of the Bureau. All of this is interesting, but Elliff's thoughts on domestic intelligence gathering are likely to be most controversial.

At the core of Elliff's position is a sharp distinction between intelligence-type operations (which are general in scope, focus on groups of politically active individuals, and continue over time collecting a wide variety of data about that group) and investigations for the purpose of prosecution. The latter involve ordinary detection, and are circumscribed by the circumstances of the crime and limited in time by the prosecutorial objective of the enterprise—discovering "did they do it?" and determining whether sufficient evidence can be obtained for prosecution. Elliff would allow the initiation of an intelligence investigation (where the immediate objective is not prosecution, but foreknowledge of violent acts so that they may be prevented) only where the group involved has a fresh track record of violence. Today a group such as the Puerto Rican nationalist FALN might be the subject of such investigations, and the Symbionese Liberation Army and the Black Panthers in the late 1960s, would be other examples. Without a record of violence, however, no information gathering could be undertaken unless there were grounds for opening an ordinary criminal investigation—some rather clear indication a group has or is preparing to commit a substantive federal crime.

This is not altogether persuasive. The sharp distinction which Elliff posits between intelligence-type investigations and investigations aimed at prosecutions is too fine for the real world of politically motivated crime. There will be instances when the FBI, or indeed a state or local law enforcement agency, will have information (a tip, some concatenation of circumstances, some unspecific references by an established informant) which indicates that a group—without a previous record of violence—may be preparing for new and lawless initiatives. This information falls short of what Elliff would consider sufficient for beginning a regular criminal investigation, and a full-scale intelligence gathering is foreclosed. Yet it would be absurd to take no action at all. Is it not more useful to think in terms of a sliding scale of information-gathering techniques which may be used in response to information which creates varying levels of suspicion as to the possibility of violence? Thus relatively non-intrusive techniques (e.g., interviewing willing respondents) may be resorted for relatively short periods in following up on relatively ambiguous indications that something is going on out there.

But whatever particular quarrels one may have with it, there is no doubting the importance of the book; whether the issue is domestic intelligence, or counter-intelligence, or foreign intelligence gathering by the FBI, Elliff has framed the issues and defined the terms in which we will all be debating.

RICHARD E. MORGAN

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The Politics of Benevolence: Revival Religion and American Voting Behavior. By John L. Hammond. (Norwood, N.J.: Ablex Publishing Corporation, 1979. Pp. xi + 243. $16.50.)

Historians and political scientists have long assumed some connection between religious movements and political turmoil in the decades prior to the Civil War. John Hammond's book provides an analytically rigorous and technically sophisticated demonstration of the nature and significance of that causal nexus in two electorally important states, New York and Ohio.

The revivals of the 1820s and 1830s transformed the consciousness and altered the behavior of those who experienced them. To the revivalists human beings were not passive instruments in the hands of an angry God, but free agents capable of saving themselves by voluntary acts of will. And those who had thus saved themselves had a correlative and divinely mandated duty to save others, to bring about God's will on earth through acts of benevolence. To free the world of sin, revivalists aimed their benevolent endeavors at eliminating whatever restrained that exercise of free will upon which individual regeneration ultimately depended. Within the sociopolitical context of the antebellum U.S., it was as inevitable as such things can ever be that those motivated by such a belief system would channel their fulsome zeal into efforts to eliminate the "sin of sins"—the institution of slavery. For human bondage negated the operations of free will and thus precluded the possibility of salvation.

In two superbly conceived and executed chapters (3 and 4), Hammond delineates these relations, specifies their predicted behavioral consequences, marshals the relevant empirical data, and operationalizes multivariate tests of his hypothesis and competing ones. The result is a compelling confirmation that what he terms the "revivalist ethos" had direct effects on political behavior. In a less tightly reasoned and more discursive chapter (Ch. 5), Hammond analyzes the transition from the self-consciously benevolent and noncompromising abolitionist politics of the 1840s to the more diversely motivated and pragmatic antislavery politics of the 1850s. Finally, in a chronologically sweeping but theoretically well-structured chapter (Ch. 6), he traces the post-1865 transformation of the ethos from a counter-normative belief system into an orthodoxy. Here, by contrasting the New York and Ohio cases, Hammond is able to specify the requisite conditions for the institutionalization of the cultural pattern.

There are some analytical soft spots in the book. *First,* Hammond has not identified the full range of impediments to free will that troubled contemporary revivalists. In addition to slavery and drunkenness, they spoke as well of "popery" and the "spirit of party." The former is important in understanding the dispositional consonance between revivalism and political anti-Catholicism; the latter is crucial to explaining the task of coalition construction that confronted Republican party builders in the 1850s. *Second,* Hammond overlooks the limits on free will inherent in the revivalist belief system. For freedom, as they articulated its definition, was simply "the right to do right." *Third,* Hammond's treatment of the Know-Nothings is unpersuasive because it is self-contradictory. While arguing that Know-

Nothingism represented opposition to antislavery more than it represented nativism, he uses the low voting support given to Know-Nothing candidates in revivalist areas as evidence of a lack of connection between nativism and revivalism. *Fourth,* he misses the growing salience of anti-southern feeling in the politics of the 1850s. That his "Percent Southern" in Ohio associates positively with membership in antislavery societies (Table 4.1, p. 75) and with Liberty party voting (Table 4.2, p. 90), but negatively with Republican voting (Table 5.4, p. 130) provides an instructive clue. *Fifth,* by ignoring both the role of the revivalist ethos in shaping the anti-Masonic crusade and its impact on the Whig party, he has missed opportunities to analyze its consequences for the moralization of politics.

While these are not analytically trivial caveats, they should not obscure my overall judgment: this is a first-rate study of considerable significance. And its importance does not depend primarily on its substantive contribution to an understanding of political abolitionism. Its significance is much broader, for Hammond has succeeded admirably in his aim to write a book that established "the force of ideas in motivating political action" (p. 17).

PAUL KLEPPNER

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Political Ideologies of Organized Labor: The New Deal Era. By Ruth L. Horowitz. (New Brunswick, N.J.: Transaction Books, 1978. Pp. 260. $19.95.)

Ruth Horowitz has written a carefully researched account of the origin, evolution and, above all, the persistence of the voluntarist ideas of the American Federation of Labor. She shows how the enduring tradition was rooted in the American ethos congenial to its antilegislative and economic orientation, reinforced by the abortive political activities by competing labor organizations such as the Knights of Labor, and justified by the hostile political climate of the early years. The terms of this unwavering stance assumed by the Federation, particularly the personal beliefs of Samuel Gompers and later William Green, were variously diluted under the Roosevelt administration, and Horowitz studiously traces the changing pattern of the American labor movement by plotting labor's reaction to several legislative measures passed during the 1930s.

The Norris-LaGuardia Anti-Injunction Bill, the unemployment insurance legislation, the National Industrial Recovery Act, the National Labor Relations Act, the Social Security Act, and the Fair Labor Standards Act, all posed a serious threat on AFL voluntarism that ultimately served to modify and alter the traditional viewpoints of the labor leadership. This happened chiefly in three ways. The substantive benefits of certain measures, notably the Norris-LaGuardia Bill, prompted the AFL to reduce the anti-statist tenor of its voluntarism since the government's role defined by the bill was not essentially inimical to Gompers' earlier sympathy for the type of legislation that enhanced the freedom of union activities. This did, however, seriously muddle the tenets of voluntarism and the result was "an ideological ambivalence which compromised the old tradition without actually destroying them."

Second, some of the New Deal measures threatened to fragment labor's organizational solidarity. The AFL Executive Council's initial opposition in 1929 to Senator LaFollette's unemployment insurance legislation invited a host of strident challenges by Socialist leaders, especially from the Wisconsin delegation, as well as by a number of unions such as the Meat Cutters, the American Federation of Teachers, the Post Office Clerks, the Cloth, Hat and Cap Makers Union, and the powerful Teamster's Union. At the 1932 convention, the Executive Council acceded to the demand and came out in support of the bill "in order to prevent it from becoming a source of serious disunity in Federation ranks." Third, although voluntarism would survive the Wagner Act, the Federation's subsequent reliance on the government arbitration had the effect of weakening the union's adamance on its tradition.

Finally, AFL's departures from a narrow voluntarism could no longer be improvisatory when faced with the formidable competition for the membership with the Congress of Industrial Organizations and its mass-production workers who passionately supported the New Deal offerings like the Social Security Act and the Fair Labor Standards Act. Horowitz identifies the CIO as "the primary factor in changing the orientation of the AFL" that had "the effect of so compromising its voluntarist premises as to almost obliterate them."

In this sophisticated chronicle of the kind of gradual changes that were wrought into the AFL's traditional ideas, Horowitz in my judgment successfully dispels the "popular" notion that the New Deal protective legislation was a response to "consistent and emphatic demands for it from the great labor union organizations of America's working class." To be sure, there are certain annoyances in this book. For example, the conspicuous absence of an explanation for Gompers' dramatic reversal of his earlier nonpartisan neutrality (Section 8, Article IV of the AFL's Constitution comes to mind), is somewhat baffling. In 1908, when labor's legislative program met with an unfriendly reception by the Republican party, Gompers decided publicly to support the Democratic presidential candidacy of Bryan. The Democratic endorsement was never an official stand by the AFL convention but the Federation's open courtship with the party had lasted well through the 1910s. Horowitz's proclivity to emphasize the Federation's adamance and to view it as an "ideological" commitment unnecessarily introduces the kind of rigidity in her analysis that in part understates the programmatic flexibility to which the Federation was prone from time to time. When one recalls, for example, the important role of labor's deep-seated suspicion of federal courts in sustaining its voluntarist tradition, such a consistently rigid view of labor's leadership position may not be warranted, or, for that matter, may not be helpful in furthering our insight.

One can also quibble with Horowitz's rendition of the so-called "popular" view that the Roosevelt policy was an attempt to accommodate labor's sentiments (with a possible exception of George Higgins). The traditional position of the Federation, occasionally moderated by changes in the external political climate, the internal schism, and finally eroded by the CIO's threat, has been a subject of serious scholarly studies elsewhere, notably John Commons, Joseph Raybeck, Selig Perlman, Philip Taft, Michael Rogin, Marc Karson, Avril Harris among others. What *is* new is the clarity and elaborateness with which Horowitz retraces the history of the labor movement during the New Deal era from one particular anchoring perspective—that of evolving and waning voluntarism among the ranks of the labor leadership. In the process, she reveals an interesting and important account of the internal intransigence and dynamics both within the AFL and within the labor community as a whole. In many ways, this volume makes a substantial and timely contribution to our understanding of the contemporary labor politics.

JONG OH RA

Hollins College

Communist Cadre: The Social Background of the American Communist Party Elite. By Harvey E. Klehr. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. xvi + 141. $9.95.)

I suspect that the initial reaction of many, if not most, political scientists to the title of this book will be skepticism. Can this be anything but a further rehash of ideological arguments cloaked in sociological jargon? Happily, the answer is yes. Klehr's book is notably free of argumentation or bias (although in this genre I am sure there will be those who can find some key words or phrases or omissions that reveal a subtle effort to mislead), and is written with clarity and a sense of style.

For the most part, the book is about what its title says—the social background of the CPUSA "elite." In an excellent introductory chapter, Klehr carefully and succinctly reviews the past work in the field; points out why his study is needed, and its goals; defines his terms (e.g., unless otherwise specified, the "party elite" are members of the Central Committee); and describes the sources and problems of his data. Not surprisingly, the data leave something to be desired, but Klehr has left few sources untapped—ranging from newly opened FBI files, to party lists, to personal interviews—and is frank about their limitations and his strategies for coping with them.

The book covers the members of the Central Committees between 1921 and 1961, and is organized by chapters on the immigrant, Jewish, black, and female components of the membership, plus membership change. The basic findings are that the initial party leadership was composed predominantly of foreign-born and first-generation Americans, who were well-educated Jewish males of Eastern European descent with white-collar occupations. That the leadership was definitely not representative of the American population was a sensitive point, and "native Americans" (i.e., not recent immigrants), including blacks, were recruited and rapidly promoted in efforts to make it so. Nor was the leadership either a stable or independent group of decision makers. Leaders (and followers) moved in and out of the party at a great rate, often because of policy dictates from the comintern.

The book has its problems. One is that the reader's natural interest in how party decisions were made and who had power is not satisfied with social background data. Obviously Klehr feels the same way, for he frequently makes points about motivations, policy decisions, leadership conflicts, etc., that are not explicit in his data; so there is some question about where this information comes from. Nevertheless, despite this methodological impurity, I found these points smoothly integrated into the social background material and generally helpful to the book.

The second (small) problem arises from the use of percentages based on very small Ns. With the total set of Central Committee members numbering 212, breakdowns into subsets of Jews, blacks, countries of origin, etc., quickly lead to Ns of 21, 12, or even 1! Klehr does not abuse this inherent limitation by over-interpretation, but the reader needs to keep alert to the difficulty.

The last problem is more serious: repetition. The facts that the CPUSA was foreign-born, Jewish, reorganized at certain points, etc., are repeated in chapter after chapter. Because Klehr writes well, this is not obtrusive within any one chapter, but over several it becomes quite noticeable. Indeed, chapters often seem to have been written to be read independently. Perhaps some of this is caused by the thinness of Klehr's data, but in any case the reader can get the major findings of the study from the introduction (18 pp.) and chapters 6 and 7 (20 pp.).

As a whole, this is a strong piece of work. Klehr sets forth relatively modest goals and achieves them with appropriate methodology and careful analysis. The writing is felicitous, and I do not expect the findings to be overturned, although evidently much remains to be done on the internal political processes of this type of organization.

PAUL A. SMITH

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Technology Transfer to Cities: Process of Choice at the Local Level. By W. Henry Lambright. (Boulder, Colo.: Westview Press, 1979. Pp. xv + 188. $16.50.)

This book is concerned with a subset of the larger issue of technology transfer, namely, technology transfer to the city as it relates to urban public services. The data base for this exploratory study is drawn from metropolitan Syracuse and Rochester, New York. Case studies focus on the political and organizational variables having an impact on decisions to adopt or reject technology at the urban level.

Contending that some technologies are less complex to adopt than others, the authors list the characteristics which make them so. The book presents a conceptual model of urban innovation that is ordinal in terms of a rejection-to-incorporation scale of technology transfer.

The book concludes with an assessment (based on the case studies) which stipulates the conditions for successful technology transfer: specifically, a local "performance gap" in some urban services providing an environment in which local entrepreneurship (both bureaucratic and nonbureaucratic) can emerge. Its importance is underscored by the contention that technology transfer is, essentially, an exercise in coalition building.

These well-written and innovative case histories provide samples of both hardware (such as the mini-pumper fire truck) as well as software (such as the Uniform Fire Reporting System [UFIRS]). However, because the study is exploratory and its data base limited, it is best conceived as a medium for generating additional research questions. It is of more particular interest to students in this area than to a larger general audience.

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Energy: The Next Twenty Years. By Hans H. Landsberg et al. (Cambridge, Mass.: Ballinger, 1979. Pp. xxvii + 628. $25.00, cloth; $9.95, paper.)

Energy in America's Future. By Sam Schurr et al. (Baltimore: Johns Hopkins University Press, 1979. Pp. xxvii + 555. $30.00, cloth; $10.95, paper.)

Notwithstanding the many issues these two volumes deliberately raise, the most intriguing may be how Resources for the Future (RFF) came to publish two such similar books at the same time. Both are RFF-managed; both are cautiously optimistic; both recommend market solutions. On the first level the answer to this publishing riddle is mere happenstance. The Mellon Foundation and the Ford Foundation independently chose RFF, viewing this think-tank (located in Washington's intellectual ghetto) as the hub of energy analysis. On the second level, the redundancy connotes a lack of healthy diversity in thinking about energy policy. The contemporaneous *Energy Future* (Random House, 1979) by Robert Stobaugh and Daniel Yergin of Harvard Business School shared the RFF studies' optimistic conclusions and market recommendations. This 1979 consensus seems excessive, all the more so in contrast to the 1974 book sponsored by the

Ford Foundation, *A Time to Choose,* which was pessimistic and anti-market. One can conjure up the image of some fickle energy policy elite that lurches from alarmism to complacency and from government interventionism to the free market much as jet-set celebrities switch favorite discotheques.

RFF's redundancy and inconstancy need not worry the reader. Either book serves as a good primer of the energy basics. Both begin with discussions of demand and supply of all fuels, giving oil the heavier emphasis it deserves, then both go on to discuss conservation, environmental impacts, and new energy sources, and finally both consider how to make decisions and what those decisions ought to be. While in both books the exposition is clear, the sheer mass of facts eventually overwhelms the reader. As with a science textbook, the novice must plow through these volumes slowly and carefully.

Recommendations abound, a feature political scientists and policy analysts will applaud. There is plenty of meat for critics, teachers and students. As mentioned above, both studies favor market solutions. The Ford book explicitly lists nine proposals starting with "decontrol oil prices" (p. 46). The Mellon books proposals also begin by recommending that prices reflect the market (p. 46). Further comparison of the books' recommendations point by point shows extensive agreement. Both urge promptly filling the Strategic Petroleum Reserve, government responsibility for research coupled with private responsibility for development, greater use of coal, prudent expansion of nuclear power, conservation, removing impediments to solar energy and concern that politicians' desire for supposed income redistribution schemes not outweigh the benefits of economic growth. Differences are few and minor. The Mellon study is more favorable toward synthetic fuels; the Ford study is more critical of the Clean Air Act.

These two books raise the question of political science's contribution to making and understanding energy policy. Introducing part 4 on decision making, the Ford book notes: "Specialists in political science, government or administration, for example, may find meager pickings. . . ." Amen. And the pickings are just as meager in the Mellon book. Why is it that political science neither contributes to, nor seems to understand, an issue area as political as energy? One might go so far as to say that to the extent there is an energy crisis, the causes are chiefly political: OPEC, the Iranian Revolution, the FEA and DOE, the Natural Gas Act of 1938, *Phillips* vs. *Wisconsin,* city housing codes, and so forth. While RFF might agree that the causes are political, it merely accepts this idea without inquiring as to why this is so or how these causes might be removed. Alternatively RFF's 1979 shift away from Ford's 1974 political solution might reflect the failure of political solutions tried since then. Synoptic plans like the 1974 Project Independence, the 1977 National Energy Plan, and President Carter's 1979 proposals have all been gutted. Delay, confusion and mismanagement have beset more modest programs such as gasoline rationing and the Stratetic Petroleum Reserve.

To what extent could these problems have been anticipated and avoided? Could not better political science have headed off these failures? Perhaps political scientists would have offered a dose of preventive medicine. Public administration could have warned of the perils of establishing a new agency like FEA, ERDA, NRC or DOE. Congressional experts could have warned of the fate of a synoptic plan when exposed to the segmented authorities and interests of Congress. Area specialists could have warned of the dangers of depending on the political stability of Iran or other exporting nations. Whatever may be the extent of political science's contribution to energy policy, it receives scant recognition in these two books.

DAVID HOWARD DAVIS

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New Dimensions to Energy Policy. Edited by Robert Lawrence. (Lexington, Mass.: Lexington Books, 1979. Pp. xiv + 233. $21.95.)

No public issue has dominated American politics more intensely over the past seven years than has the energy crisis. One might expect, therefore, to see scores of political scientists veritably feasting on the subject. Not so. Curiously, energy policy has still to attract extensive, sustained political analysis of a scholarly sort, at least judging from the contents of the profession's leading journals. Thus, any publication that can help draw together recent work will inevitably be read not only with interest but with high expectations. Robert Lawrence has edited such a compilation—but the result is spotty.

Granted, the energy predicament is multifaceted, affecting our social and political institutions at all levels. But Lawrence's compendium ranges awfully far afield for a volume of its size. No fewer than 18 different articles are packed into some 200 pages of text, including notes, such that much of the material is simply too condensed to permit more than superficial treatment of highly complex questions. (A section entitled, "Pluralism and Energy: Carter's National Energy Plan," for instance, spans a mere 8½ pages!) Further, because many of the essays address narrowly gauged side-issues of the energy problem, the central disputes often don't get the attention they deserve.

Well over a third of the book is devoted to the state and local context. Conceivably, this emphasis could be justified if the focus were on: the pivotal role of the states in implementing federal energy development and regulatory policies; the sensitive position of local governments in siting pipelines, refineries, and nuclear plants; the record of state utility-rate reform; the connection between "soft" energy alternatives and "decentralization"; the natural gas pricing policies in the southwestern states (that provided till now unique models of how markets operate without price controls); the fact that a few states took steps that actually influenced the national agenda, or that, as in the case of California, were in some respects more significant than certain federal initiatives; and other such considerations beckoning for close scrutiny. But with the possible exceptions of one brief report on state policy innovations and a couple of other pieces, these are not the kinds of concerns with which the discussions of state-local "dimensions" deal. Instead, we are apprised of a "framework of analysis" that town planners could use to weigh the local fiscal effects (that is, effects on local employment, services, land use, and budgets) of future coal conversion projects; of the essential nature of state energy forecasting; of how "fragmented" and "disorganized" (very generally speaking) are the energy "planning" and "coordination" efforts in Arizona, Colorado, Nevada, New Mexico, and Utah; of the need for an energy policy for Indian lands; and so on. Topics like these are not irrelevant, but neither are they exactly at the core of the current national controversy.

Some parts of the book, however, are highly salient to anybody following closely the national debate. An essay by Gregory A. Daneke contains an account of how government subsidies for nuclear research effectively discriminated against other options, helping to create the present technological lag in solar energy. Lenneal J. Henderson, Jr. (DOE) looks at an oft-neglected aspect of energy price increases—their grossly regressive impact—and discusses utility rate adjustments that could partially alleviate the problem. Charles O. Jones provides an insightful review of the congressional response to the National Energy Plan (NEP) during the first (though not the second) session of the 95th Congress. The fate of the president's original energy package is also examined by David Howard Davis. Davis' short article purports to analyze interest-group politics ("pluralist theory") in the conflict over NEP. I found most of his analysis too skeletal to be illuminating and, on some points, just plain wrong. (Example: Contrary to Davis' assertions, the notorious natural gas compromise did *not* involve deregulation of both "new" and "old" gas. Technically, the compromise does not even deregulate all newly produced gas. That, as a matter of fact, was one of the reasons for all the fuss in Congress.) Nonetheless, Davis does touch on an important feature of the legislative struggle in 1977–78: its sharply partisan nature. The role of party (or, more basically, ideology) in energy politics has been generally underestimated in the prevailing commentary. It is well worth looking into.

Perhaps the best chapter is that by Alfred R. Light, who does everybody a special service by tracing carefully the complicated legislative origins of the Carter plan—that is, the links to prior presidential and congressional initiatives, and even to some interesting experiments at the state level. Not the least of this essay's virtues is that it helps dispel the seemingly widespread supposition that NEP was lifted wholesale from the Ford Foundation's study *A Time to Choose*, or that Schlesinger's task force simply conceived the plan out of thin air.

Still, despite these contributions, Lawrence's collection generally leaves us more conscious of what political analysts should be searching for than impressed by what they have already found. The agenda is vast, but certainly among the many questions in need of more intensive investigation (or reinvestigation) would seem to be: The oil lobby—what is it really like these days, and how is it doing? Public opinion—is it making any sense? Presidential leadership—where did the Carter administration miscalculate? (The popular cant about the ineptitude or inexperience of the White House and the Energy Department may well have merit. One cannot say, however, that those who incessantly stress this theme typically take the trouble to substantiate it adequately, or to put it in proper political perspective.). Congressional immobility—what's going on? How much of it is due to parochialism, to presidential bungling, or to deepening ideological cleavages? American foreign policy—how are the geopolitics of energy shaping it? Finally, it remains difficult to understand *the politics* of the major national energy proposals of recent years (NEP, for instance) without first analyzing, rather thoroughly, *the policies* themselves, especially the distribution of their costs and benefits.

Overall, *New Dimensions of Energy Policy* is not a book oriented along these lines. That may be, to some extent, a reflection of the state of the art.

PIETRO S. NIVOLA

University of Vermont

Neither Athens nor Sparta? The American Service Academies in Transition. By John P. Lovell. (Bloomington: Indiana University Press, 1979. Pp. xviii + 362. $17.50.)

John Lovell has been an avid student of the American service academies throughout his professional career. A graduate of West Point, he wrote a dissertation on the process of political socialization at that institution for his doctorate; he taught at the Naval Academy and the Army War College; and he has made numerous research and lecturing trips to the other academies. A student of organizational theory, he is quite qualified to address the subject of innovation at the service academies, and those who have heard that the present book was "in the works" have been awaiting its arrival with a more than ordinary degree of interest. They will not be disappointed, for this is a fascinating study, rich in historical detail, analysis and insight.

Lovell begins by noting that the several American service academies (West Point, Annapolis, the Air Force Academy and the Coast Guard Academy) have experienced a number of changes in the past 40 years. He describes these changes, and then sets out to analyze the process of change at each academy, offering generalizations regarding the innovations that transpired and the forces limiting and containing them.

World War II and the emergence of a vast postwar military system appear to have created an atmosphere, and some pressure, for change, West Point, an institution that had seen little innovation for a period of over 100 years, was the first to bend. Generals Maxwell Taylor and Garrison Davidson, superintendents there in the late 1940s and mid-1950s, respectively, appear in Lovell's account as central actors in two waves of curriculum revision and academic reform. Simultaneously with General Davidson's tenure at West Point, Colonel Robert McDermott, academic dean at the newly created Air Force Academy, fought with some success to accomplish similar academically oriented objectives and to create a climate for "Athenian" (that is, essentially intellectual) values to flourish. Shortly thereafter Admirals Frank Leamy and Willard Smith at the Coast Guard Academy and, several years later, Admiral James Calvert at Annapolis brought their institutions in line with West Point and Colorado Springs. Electives, academic majors, a slight deemphasis on things "Spartan" (that is, purely military), and, at the Naval Academy, the hiring of large numbers of civilian academics, were among the more noteworthy of the innovations in the quarter-century since the end of World War II.

Nonetheless, resistance to change was substantial: "Let a hundred flowers bloom!" some midshipmen quipped, peeved at the stinginess of the changes (p. 184). Despite widespread (but insufficiently specific or firm) criticism directed at the hazing process by reform-minded leaders at each of the service academies, that process survived with only modest amendments at all the institutions, reflecting the survival of "Spartan" values. Similarly, despite its persistent failure and abuse (by academy officialdom), the honor code continued everywhere to be used to "maintain order and discipline" in addition to its primary function (p. 63). In this regard as in others, General H. R. Harmon, the venerated first superintendent of the Air Force Academy, comes across in Lovell's account as the preeminent traditionalist and opponent of the "Athenian" model. (It is a mark of Lovell's objectivity that he is gentler with Harmon than other writers might have been.) Lovell reminds us that nineteenth-century and early twentieth-century innovations, such as they were, were invariably followed by decades of reaction and stagnation, and he offers some well-reasoned thoughts on the future of innovation at the service academies.

He concludes that little change will occur if the ultimately controlling external environment is hostile towards, or deeply divided about, organizational or academic change; that the superintendent's length of tenure and the character of his relations with other key academy officials and governing bodies are central to the success of innovations he seeks to make; that the General Accounting Office can often play a valuable role in facilitating the intended changes; and that innovations tend to spread from the initiating academy to all others. He predicts more curriculum reform, the creation of academic majors at West Point, and an increase in "free time" and privileges for cadets and midshipmen. On balance he seems to favor, but does not regard as likely, the eventual transformation of the service academies into two-year post-graduate schools.

Lovell presents a detailed, adeptly drawn, and fascinating account, and an analysis that seems well structured, thoughtful, and quite sensible. His book is a valuable contribution to our understanding of organizational innovation in general, and U.S. service academies in particular.

PETER KARSTEN

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The Art Museum: Power, Money, Ethics. By Karl E. Meyer. (New York: William Morrow and Co., 1979. Pp. 352. $15.00.)

The Subsidized Muse: Public Support for the Arts in the United States. By Dick Netzer. (New York: Cambridge University Press, 1978. Pp. ix + 289. $14.95.)

The two books reviewed here are part of a series analyzing American public arts policy sponsored by the Twentieth Century Fund. The other two volumes, a study of indirect arts subsidies by Michael O'Hare, and an analysis of appropriate objectives for government support of the visual arts, by Edward Banfield, are forthcoming.

The Netzer and Meyer volumes are important contributions to the growing literature dealing with government support of the arts, and are must reading for anyone seriously interested in the subject. The books share two common themes: first, that arts policy is dominated by a small group which looks very much like G. William Domhoff's ruling class, and second, that indirect public support of the arts, through tax policy, has been more important than direct subsidy.

On the first point, Meyer quotes Robert Moses on the Metropolitan Museum of Art: "[It] is still a club—exclusive, dignified, public-spirited, traditional, not fully aware of the significance of modern trends, relatively unimpressed by public opinion, removed from local pride and pressure. . ." (p. 94). Meyer notes that the Board of the Metropolitan has had only two artists of any note as members in its 108-year history, while no professional art historian has been a member. A 1965 survey of art museum trustees found that 60 percent were Ivy League graduates, 40 percent Episcopalians, and one-third bankers or financiers (Meyer, pp. 224–26).

Even though direct public subsidies to arts institutions have burgeoned from $21 million in 1965 to $282 million in 1975 (Netzer, p. 3), grant recipients are often not accountable to their public benefactors. The Metropolitan's meetings and minutes, for example, are not open to public scrutiny. Indeed, in a fit of anger after chances for the proposed Annenberg media center were destroyed by adverse publicity, the Metropolitan board barred city representatives from executive sessions (Meyer, pp. 119–25). Even the Smithsonian, a hybrid private/public institution, has a record of "eluding statutory requirements in its distribution of federal funds" (Meyer, p. 53).

Direct subsidies go for the most part to the biggest arts organizations, like the Metropolitan, which are most likely to be dominated by the socioeconomic elite. Artists themselves receive only a small share of grant funds. In fiscal year 1974, $2.5 million of $67.6 million in National Endowment for the Arts subsidies went to artists. Further, the "endowment has accorded relatively low priority to experimentation, the avant-garde, and the wholly new in artistic production at fully professional levels" (Netzer, p. 74). Netzer concludes, in the case of another giant Metropolitan, the opera, that "subsidy of the Met will not bring it to the poor, expand its audience, cause it to do radically different things. . ." (p. 166). Yet the Met and its institutional brothers and sisters are guaranteed a place at the public trough despite these shortcomings. "The funding agencies are as much locked into many of their existing grantees as the grantors are locked into the grants" (p. 175). While the large institutions have cried out in outrage over the "populism" of the Carter administration, no significant shift in funding patterns is yet noticeable.

The second common theme, the importance of indirect tax support of the arts, cannot be underestimated. Netzer estimates that this "indirect subsidy," "provided by deductible gifts to the arts from federal, state, and local income enjoyed by donors because of their gifts—amounted to at least $400 million in 1974" (p. 4). As Netzer notes, this total may actually be much larger, and O'Hare's forthcoming volume should shed further needed light on the topic. In the case of the visual arts, as Meyer observes, tariffs have also been a crucial consideration. J. P. Morgan was able to have a provision inserted in the 1909 Payne-Aldrich tariff enabling works of art to enter the U.S. duty-free (p. 32), which allowed a flood of foreign art to enter American museums.

Besides probing these two common themes, these two books examine divergent areas of great moment. Netzer is an economist concerned with the reasons for public arts subsidies and their impact. Meyer, a journalist and political scientist, probes "those features of the art museum that touch on public policy" (p. 11).

Netzer relies principally on various types of economic data (plus 24 interviews), which he commendably admits are "fragmentary evidence," forcing him into "using guesses and sometimes arbitrary estimates" (p. 7). Would that all of us social scientists were so appropriately modest! Netzer's astute questioning, however shaky some of his evidence may be, makes his an important contribution. For example, he does not accept the 1966 Baumol-Bowen study, *Performing Arts: The Economic Dilemma* (another Twentieth Century Fund study) as holy writ. Where Baumol and Bowen argued that secular trends might require public subsidy just to maintain artistic organizations, Netzer cogently takes issue with several of the foundation stones of this interpretation (pp. 28–32).

While lack of space precludes further attention to this topic here, Netzer's treatment of the above topic makes his book an indispensable study in arts policy, as does his discussion of the economic nature of arts subsidies (pp. 15–28). He concludes that diverse public subsidies have "made possible very large increases in earnings of artistic and supporting personnel . . . facilitated audience access to the performing arts . . . and fostered some increase in output in terms of the number of opera and symphony performances" (p. 108). Throughout the book, Netzer poses provocative questions about the values behind an arts policy.

Meyer's conversational style and liberal use of photographs make his book a pleasure to read; he interviewed 300 respondents to gather data. (Strangely enough, he doesn't cite the Netzer volume, published before his own.) Meyer's picture of the economic market in *objets d'art* is a trenchant commentary on personal and institutional corruption and distortion of the original values of art museums (pp. 165–74). Meyer concludes by wondering whether the U.S. really has something which could be called a national arts policy and whether it should have one. In this respect, both the Meyer and Netzer books represent a welcome return to writings on public policy which raise normative as well as empirical questions.

DOUGLAS M. FOX

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The GAO: The Quest for Accountability in American Government. By Frederick C. Mosher. (Boulder, Colo.: Westview Press, 1979. Pp. xx + 387. $24.00, cloth; $12.00, paper.)

Cases in Accountability: The Work of the GAO. Edited by Erasmus H. Kloman. (Boulder, Colo.: Westview Press, 1979. Pp. x + 254. $15.00, cloth; $7.50, paper.)

It is not surprising that Comptroller General Elmer B. Staats, whose administration of the U.S. General Accounting Office (GAO) has been characterized by unusual dynamism and innovation, would like to see more widespread understanding of the vital, multifaceted role that this agency plays in the federal government. It is a role of such expanding dimensions that it has an increasing impact upon state and local units of government, contractors, other recipients of federal funds, and even the accounting profession per se. In calling upon the National Academy of Public Administration to make a study of the GAO, Staats hoped that it would yield a wider perception of the office through the production of a history and description of the development of his agency. This frame of reference differs markedly from that of my own *Watchdog on the Potomac* (University Press of America, 1979), published several months before the two volumes under review and which reflects a more critical approach.

Fortunately, Frederick Mosher, who directed the study under the guidance of a panel designated by the National Academy, was able to obtain agreement that the resultant book should comprise two interdependent parts: a history and an analysis of the current situation. But, it was still understood "from the beginning that this study should be expository, not evaluative." Hence, while seeking to explore what he considers to be the basic issues and arguments advanced pro and con, the author explicitly eschews any recommendations.

These constraints, however, did not preclude Mosher from producing a highly informative treatment of the GAO that fully portrays its antecedents and the broad sweep of its activities. Dividing the office's evolution, after its establishment in 1921, into "three GAOs," the author effectively depicts the manner in which the several comptrollers general have left their imprints upon the structure, priorities, approach, and relationships of the organization. The second part of the book, carrying the rubric "Emerging Roles of the GAO," assumes a more analytical quality in dealing with the governmental and social milieus within which the agency operates as well as various facets of its organization, processes, and personnel. Mosher's expressed hope that his study will contribute as much to the understanding of American government as it does to the understanding of the GAO may appear to be overreaching. Yet, he has not fallen completely short of this mark.

Notwithstanding impressive research, the Mosher study has shortcomings that cannot be attributed entirely to the author's nonevaluative approach. Some points are glossed over and, in other instances, readers who might have been tantalized by a particular factual statement are left to their own resources to make any judgment. For example, readers might have been apprised more fully as to the price which the "quest for infallibility," that has generally characterized the preparation of GAO reports, exacts in terms of timeliness and utility of the reports. The "strategy and tactics" of GAO's dealings with the Congress could be perceived in more realistic terms if some of the relations with particular members had been explicated. In stressing the vigorous manner in which the comptrollers general had "defended their independence," Mosher fails to explain why it was not until 1977 that the comptroller general discontinued the highly questionable practice of leaving it up to committees and individual members to determine when reports prepared at their request might be distributed. Mosher tends to minimize GAO's difficulties in gaining access to executive agency records; yet the comptroller general has been importuning Congress to strengthen his authority on that score, and the pending General Accounting Office Act of 1979 would authorize the comptroller general to bring court action to compel the furnishing of demanded documentary materials.

But what is especially regrettable about the Mosher treatment of the subject is that possible resolutions of the many issues which he raises and probes, whether in depth or casually—are, pursuant to the limitations placed upon the study, left hanging in limbo. This at a time when there is such an urgent need for the crystallization of a framework for the totality of the GAO's activities. This need is accentuated by a host of factors: the unusual mix of legislative and executive functions performed by the comptroller general, the establishment of inspectors general in many of the executive agencies, questions as to both the independence and accountability of GAO, potentialities for better coordination and use of staff resources and support agencies available to the Congress, and the continuing thrust for expansion of GAO's role whether initiated by the Congress or the comptroller general. Mosher and the National Academy of Public Administration—buttressed as it was by the participation of a panel of distinguished citizens—were in an enviable position to make a far more constructive and positive contribution to shaping the future of the GAO. This would have been particularly timely in light of the fact that a new comptroller general will be selected in 1981, when Staats' term comes to an end.

Cases in Accountability: The Work of the GAO, a companion to the Mosher book, is designed to illustrate the diversity of GAO projects and to afford the reader insights into the rationale and content of the studies, the manner of conducting them, and developments that followed. Most of these case studies were written by GAO professionals from the divisions in which the original work was performed. They range from such noteworthy subjects as "The Seizure of the S.S. Mayaguez" to routine cases that would rarely elicit public attention, but are included to demonstrate the range of problems with which GAO deals. The collection is concerned primarily, but not exclusively, with what GAO classifies as "reports."

Because of the plethora of GAO reports—839 to Congress, committees, and members and 297 to federal agency officials during the fiscal year 1978—this volume is most welcome as a convenient means for obtaining insights into a cross section of the end products of much of GAO's efforts. Guidance in perusing the cases is provided by questions which Kolman raises in the introduction as well as by specific questions following each case.

Like the Mosher volume, this book sidesteps controversy by leaving evaluations to the reader. Without detracting from the utility of this work, one can contend that it would have been even more meaningful if the cases had been subjected to critical evaluation by individuals in no way associated with the GAO and who, for that matter, might have been given the opportunity to make their own selection.

JOSEPH POIS

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Collective Insecurity: The United States and the League of Nations During the Early Thirties. By Gary B. Ostrower. (Cranbury, N.J.: Bucknell University Press, 1979. Pp. 287. $17.50.)

United States foreign policy between the end of World War I and the American entry into World War II is commonly characterized as isolationist, and the lack of American involvement with the League of Nations system is frequently identified as a manifestation of this isolationist orientation. These arguments are challenged by Gary Ostrower in his study of American foreign policy toward the League of Nations during the early thirties. Focusing primarily upon the Manchurian crisis of 1931–1933, Ostrower argues persuasively that there was an "internationalist impulse" in United States policy during this period, but the impulse was never strong enough to be translated into effective policies vis-à-vis the League. The result was a vacillating American position toward the League, which contributed significantly to the inability of the organization to confront Japanese aggression and to the failure of the League.

Ostrower also examines United States policy toward the League system in three other areas: the appointment of a permanent representative to the League, a plan which ultimately failed; the American decision to join the International Labor Organization; and the unsuccessful efforts to join the Permanent Court of International Justice. In each of these areas, Ostrower establishes the existence of an internationalist thrust among key American political leaders and private groups, but the more powerful isolationist mood of the country prevented close association and cooperation between the United States and the League.

Ostrower's book provides interesting insights regarding not only American foreign policy during the interwar years but also the activities of the League of Nations. The author fails, however, to fulfill the promise on the dust jacket of the book, which states that the study "illuminates for the contemporary reader . . . recurring problems in American foreign policy and their relation to both official and public opinion." While illumination is possible, the reader must reach inferences on his own; Ostrower does not provide them.

THOMAS R. HENSLEY

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Watchdog on the Potomac: A Study of the Comptroller General of the United States. By Joseph Pois. (Washington, D.C.: University Press of America, 1979. Pp. viii + 377. $11.50, paper.)

The General Accounting Office, which the comptroller general heads, is an anomalous, important, and unfamiliar agency in the legislative branch. From its inception, the GAO has been vested with legislative, executive, and judicial functions. To fulfill some of them, it must have institutional independence. Yet, as a legislative staff service, it must be responsive to Congress. Since World War II, the GAO has transformed its auditing and other roles and its personnel. Recently it has been given additional divergent responsibilities. To encompass this complex and dynamic subject within a book of reasonable length poses a formidable task. Joseph Pois brings impressive qualifications to this endeavor. Besides an eminent academic career, he has had over three decades of experience as a high-level administrator in various private, state, and federal organizations. And he was a consultant to the comptroller general for nine years.

Watchdog on the Potomac evolved from a study which Comptroller General Elmer B. Staats requested Pois to make on trends in the GAO's program, operations, and relations with other government institutions and the public. In executing this assignment, Pois had unrestricted access to GAO personnel and records. Also, he interviewed members of Congress and their staffs, executive branch officials, and defense contractors. In revising his initial manuscript, Pois used data he acquired while doing an analysis of the GAO for the Commission on the Operation of the Senate.

The resulting product is a comprehensive, descriptive, impressionistic, and prescriptive study. Opening with an overview of the background, contemporary setting, and nature of the GAO, it treats the GAO's relations with Congress and the executive branch. After considering restrictions on access to information by the GAO, it discusses the evolution of GAO audits and problems in their production and use, explains judicial and legal advisory functions of the GAO, traces GAO concern with procurement activities, and describes the expanding GAO involvement in financial management of the government. Pois employs case histories liberally to illustrate points and afford insights. He interlards evaluative statements throughout the text and concludes with a strong recommendation for an overall reassessment of the GAO.

Judged by the author's goals, this study is an excellent piece of work. Only minor strictures apply, such as the absence of an index and bibliography or infrequent lapses from an otherwise readable style. From the standpoint of advancing the literature of political science, however, an analysis of the GAO more infused with organizational theory would be desirable. Since World War II, the GAO has undergone a remarkable metamorphosis. From preoccupation with "preauditing" the legality of agency transactions, it has moved to comprehensive auditing that considers economy and efficiency of administrative operations as well and has pioneered program evaluation. In the process, the GAO has reduced (from 14,904 employees in April, 1946 to 6,100 in September, 1953), has diversified, and has upgraded its staff. Tensions exist within the GAO between the multidisciplinary professional personnel recruited to implement this new orientation and the top supervisory echelons dominated by accountants. Such developments invite the application and testing of propositions derived from the works of Anthony Downs, James D. Thompson, and other organizational theorists. In any case, *Watchdog on the Potomac* provides a worthy, comprehensive, and up-to-date introduction to an important federal agency that is too little known to the public and even to many political scientists.

KENNETH KOFMEHL

Purdue University

The Past and Future of Presidential Debates. Edited by Austin Ranney. (Washington, D.C.: American Enterprise Institute, 1979. Pp. 226. $5.75, paper.)

The "debate" in the book over the meaning and impact of presidential debates is at least equal to the 1960 and 1976 debates themselves. On the negative side, Evron Kirkpatrick asserts that debates reinforce the power of entertainment values in politics, encourage choices for the wrong reasons, accelerate the trend to personalism, contribute to the dismantling of parties and discourage appropriate attention to the institutional aspects of presidential contests and the presidency. Nelson Polsby maintains, "In their present format presidential debates are uninteresting, uninformative and unedifying. They are not worth preserving and they are not worth prohibiting. The potential for civic enlightenment in audiences such as debates typically command is so great, however, that it is worth trying to figure out how to transform them into something more real and more eventful" (p. 186).

On the other hand, James Karayn, former director of the 1976 debates, believes that debates are in the interest of the American public as the best means of direct communication between candidates and the public in the face of weakening party affiliations and fuzzy issues, candidates and policies. He therefore proposes that an independent commission organize and sponsor a series of debates and other nationally televised candidate appearances to be required of major party candidates. Likewise, columnists Jack Germond and Jules Witcover suggest that the decision of whether to participate in debates should be taken out of the hands of the candidates, and away from their yardstick of self-interest, and should be somehow institutionalized.

Between the debate on debates are papers by Democratic and Republican presidential strategists in 1976. The underlying theme, as the editor suggests, is that the candidates' decisions to participate were made "in response to their perceptions of their strategic needs, not as a result of disinterested reflection upon how best to bring issues to the people" (p. ii).

Perhaps most interesting for political scientists is the research report on the impact of the 1960 and 1976 debates, prepared by Steve Chaffee and Jack Dennis. They present a review of previous work and relevant portions of their own 1976 panel study of Wisconsin voters. Their conclusions suggest that the debates contribute significantly to the information voters have about the candidates. The debates reach out to a larger, less interested public, one that is more accustomed to watching television than following politics. Also the debates were rated as most helpful in judging the personal qualities of candidates, but less helpful in presenting new issue information. However, evidence was offered of a relationship between high exposure to the debates and consistency between candidates and voters' issue positions. The researchers conclude that the informational aspect of the debates contribute to "rational" voting. "Given a more economical mechanism for sorting out the relevant information, a great many people will respond to what the candidates are actually presenting. Cognitive learning under such circumstances becomes both possible and actual" (p. 99).

Significant questions and issues relating to campaigns and elections were not addressed in the book. The debates were approached as if they were the only events in their respective campaigns. Almost no attention was given to how the debates interact with other party, candidate and issue-related strategies in the campaign, or other media events, for that matter. Clearly, debates need further investigation in the context of the larger campaign. The impact of joint candidate appearances on the political system was not addressed, with the exception of isolated comments on "support for the system," party, and presidential qualifications. Also, greater research focus must be directed to how debates might bring out and help voters assess potential for presidential performance, in such areas as administrative talent, understanding of problems, political leadership, negotiations skills, and so on. Finally, the potential of candidates for other offices engaging in similar joint appearances was not even mentioned.

A number of other interesting issues came out of this book, which actually grew out of a conference on the impact of and the past and future of debates: the evolution of Section 315 and its relationship to debates; the nettlesome questions of debate format, ranging from the candidates' desire to enhance strategic considerations, to critics' desire to have "true debates" instead of "joint appearances"; the relationship between televised joint appearances and federal financing of presidential campaigns; debates and other forms of free broadcast time in campaigns; and who should be responsible for organizing any joint appearances. These and the other issues posed by the reintroduction of presidential debates in 1976 will continue to be with us in a media era, particularly when presidential selection is surrounded by political change and a series of "crises" in different issue areas.

ROBERT AGRANOFF

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Folded Lies: Bribery, Crusades, and Reforms. By W. Michael Reisman. (New York: Free Press, 1979. Pp. ix + 277. $12.95.)

Bribery as a central means for gaining and exercising influence in modern political life has nowhere received so sophisticated and systematic analysis as in this insightful book. Michael Riesman's account eschews the popular, flavor-filled approaches whose points of departure are taken from individual indiscretions, localized structural inadequacies, and episodic deviations from rectitude. Riesman provides instead a unified explanation for the persistence of business bribery, crusades mounted against it, and the paucity of reforms enacted to combat it.

Academic inquiry into the subject of bribery has been plagued forever by a series of familiar paradoxes. Governments universally enact prohibitions against bribery in the conduct of corporate enterprise; yet business as usual features selective advancement of bribes to secure advantage for private firms. All societies affirm generalized belief systems that include condemnations of bribery, at the same time that their applied rules of conduct embrace the bribe as standard operating procedure. Political crises precipitated by revelations of bribery engender crusades for change that result in socially deleterious outcomes more often than meaningful structural reform aimed at the original abuses. For Riesman, the contradictory phenomena associated with the practice of bribery are instructive rather than mystifying, as they direct inquiry to the larger social and political settings within which bribery flourishes and is largely impenetrable to crusades for reform.

What marks this study as distinctive and establishes it as an important contribution is the theoretical framework the author constructs in the initial chapter, which is then methodically applied to various aspects of business bribery in succeeding chapters. Riesman draws the analytic distinction between a society's "myth system" and its "operational code." The myth system expresses a clear and complete set of rules and prohibitions in idealized form, embodying key values originating from social elites and providing all members of society easily understood normative guidelines on how to act. The operational code discretely specifies a contrary set of behavioral norms and the conditions under which they may be invoked as a private system of public law in stark deviation from the myth system. It is only within a class of entrepreneurs Riesman labels "operators" where knowledge of the private code is shared and where ability to manipulate the code is institutionally located. At the operational level, the combination of capitalism's essentially amoral market exchange system and liberal democracies' limited state apparatus yields opportunities for private gain and institutional advancement through extension of bribes that are normally neither regulated, monitored, nor effectively punishable.

Inevitably, however, discrepancies appear between idealized prohibitions against bribery and practical necessities requiring bribe exchanges within and between socially complex institutions. Such discrepancies have long been tolerable at elite levels from which operators are drawn. But, to the extent they become publicly visible at mass levels, inconsistencies between professed beliefs and actual practices introduce ambivalencies and anxieties among people unfamiliar with the operating code, and this subject population is capitalized upon by operators to create dependency relationships through the rationalizations and justifications offered for the apparent discrepancies. Thus social control is accomplished, in Riesman's view, by an informed and active elite corps of operators who wend their way between ideal prescription and practical necessity.

Drawing upon numerous cases from the 1970s, Riesman applies the theoretical framework to bribery in domestic United States politics to show how judicial and legislative officials selectively allow bribe payments for customary and necessary ways of doing business, all the while professing homage to the myth system and its prohibitions. One full chapter is devoted to a discussion of the relationship between the operational code and various types of bribery, with the different forms and impacts of transaction bribes, variance bribes, and outright purchases reviewed. Perhaps the most powerful application of Riesman's schema is to the conduct of a host of multi-national corporations whose global reach extends beyond the effective control of the nation state. Whether Boeing, Lockheed, or ITT, modern multi-national corporations are portrayed as having near-autonomy in their ability simultaneously to reinforce the myth system rhetorically and undermine it in practice through the purchase of business advantage.

From Riesman's perspective, campaigns launched against bribery practices take the form of either crusades or reforms, and neither offers much prospect for meaningful change. Indeed, crusades may result in greater harm than the bribery they are targeted against because they are susceptible to elite cooptation and they most often result socially in reinforcement of the myth system and politically in further entrenchment of elite dominance. While reform is aimed not at changing the myth system, but instead at the operational code, Reisman is at pains to offer even one example of meaningful reform over the past century. In this respect he is straightforward about this being a study of the politics of social stabilization rather than of social change.

The theoretical and analytical power of this book is to some degree offset by a maddeningly elusive base of evidence. The selectively drawn cases amply illustrate the author's argument, but the issue here is what confidence the reader can place in examples drawn without regard to standards of reliability or even validity, to say nothing of corroboration from multiple sources. However, such is the perennial problem for any serious researcher who would consider the question of bribery, shrouded in the veil of secrecy and camouflaged from public view, to be worthy of scholarly analysis.

Riesman's work will remain must reading for students of bribery, corruption, and political pathologies more generally because of its theoretical richness and insight. It also can be recommended with confidence to students of white-collar crime, public law, and public administration.

DAVID J. OLSON

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Legislative Reform: The Policy Impact. Edited by Leroy N. Rieselbach. (Lexington, Mass.: Lexington Books, 1978. Pp. vii + 253. $17.00.)

The academic literature on Congress in the 1970s has lagged considerably behind the panoply of changes that have shaken Capitol Hill during the decade. I still receive manuscripts which are based in whole or in part on studies either written before the wave of reforms and membership shifts transformed the congressional landscape or which ignore their effects; no doubt many of these new articles and books will travel through the pipeline and emerge six months or a year from now perpetuating generalizations that are at the least questionable for the contemporary House and Senate.

The lag between congressional change and academic analyses is in part understandable and defensible. Political scientists must resist the impulse to exaggerate the importance of what might be a transitory rules change, and must wait for some time to pass to assess the policy consequences of reform, or to construct meaningful theories. But part of the lag has been caused by a failure of scholars to describe and analyze many important changes. (This remains true for, for example, the 1977 Senate committee reorganization.)

Leroy Rieselbach's collection of short articles, *Legislative Reform*, is a welcome indication that a sizable number of congressional scholars are now focusing directly on the significant changes that have reshaped Capitol Hill and state capitals in the past 10 years; more changes are being analyzed, theories proposed and consequences assessed. *Legislative Reform* consists of 17 articles, averaging 14 pages apiece. Articles examine the effects of reform on a variety of committees and subcommittees in the House; the importance of staffing changes; the impact of the Budget and Impoundment Control Act; openness and campaign finance reforms in Congress; and reform and performance in state legislatures. Rieselbach provides a brief overview, and Shepsle and Rohde contribute an intriguing beginning to a theory of reform and policy change.

The chapters on reforms in committees and subcommittees in Congress provide much information on the internal dynamics of reform and its internal consequences for such panels as House Banking, Armed Services, Agriculture, International Relations, Rules, Ways and Means, and Commerce. There is comparatively little on the impact of reform on policy, though the chapters by Catherine Rudder on the Ways and Means Committee, and by David E. Price on oversight in the House Commerce Committee do focus on outputs. This emphasis on internal change is understandable. It is still too early to go beyond tentative generalizations about the policy consequences of congressional change, and it is enormously difficult either to sort out the variety of interrelated changes, or to relate them to policy outputs—much less to assess or to measure qualitatively policy changes themselves. Nonetheless, one gets a sense that much of the description of internal change present in *Legislative Reform* had been covered in earlier studies; the chapters here give but limited additional insight.

This collection has both strengths and weaknesses. Its strengths make *Legislative Reform* most useful for a graduate seminar on Congress and change. It is short enough and contains enough articles to give graduate students an overview of what kinds of things political scientists are now exploring in the domain of reform. The numerous chapters pack in a large number of hypotheses which could usefully be discussed and dissected in a seminar, and their range provides a basis for analyzing methods and approaches to the study of reform and change. The chapters are readable and admirably concise. A high-priced hard-cover volume which lacks an underlying thesis or theses, *Legislative Reform* is clearly not aimed at an undergraduate audience.

The book does have some value to the researcher. Many insights are confirmed and hypotheses extended. But a book which is a series of uniformly short pieces unsystematically covering a variety of topics cannot be a striking find for the congressional scholar—particularly since many of the chapters repeat data and findings that their authors have published elsewhere. On balance then, *Legislative Reform* is a moderately useful addition to a literature which, while showing signs of maturity, still needs development and refinement.

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Defense Policy and the Presidency: Carter's First Years. Edited by Sam C. Sarkesian. (Boulder, Colo.: Westview Press, 1979. Pp. xv + 341. $18.00.)

This volume contains nine chapters, five of which are revised versions of papers presented at the 1978 annual meeting of the Midwest Political Science Association, and one which was delivered at the 1978 annual meeting of the International Studies Association.

The essays, which focus on the Carter administration's approach to defense issues during its first two years, fall into three general categories: First, an introductory chapter by the editor, Sam C. Sarkesian, and a chapter by Thomas A. Fabyanic, outline the major organizational and strategic issues that faced the new administration. Second, four chapters concern themselves with the Carter administration's approach to these problems during its first two years: Vincent Davis examines the president's personality and leadership style; in two chapters, Lawrence J. Korb reviews the organization and function of the Carter National Security Council and the policy shifts of the new administration; and Doris A. Graber treats policies regarding U.S. overseas intervention. In the third category, foreign perceptions of the new administration and the reaction to its policy initiatives are the subject—James A. Linger discusses the Soviet and NATO perspective, while George P. Jan treats China and Japan. The book concludes with a brief summary and critique by Sheldon Simon.

Four of the essays deserve brief comment: Lawrence Korb's detailed analysis of the FY 1979 defense budget depicts clearly the policy shifts the administration decided upon during 1977: scaling down the Ford administration's plan for increasing defense expenditures; modifications of strategic forces, including cancellation of the B-1 and a slowdown in MX development; increased emphasis on European defense and the readiness of American forces in NATO; and the downgrading of naval projection forces. This method of analysis reveals the *actual* shifts in policy, too frequently obscured by political rhetoric; it is an approach that should be more widely employed by policy analysts. In an interesting essay, Vincent Davis argues that Carter's inexperience, compounded by personality characteristics and an engineering perspective, has resulted in an approach best described as "speak stickly while carrying a big soft" (pp. 98–99). Though this judgment now represents the conventional wisdom on President Carter, the analysis might have treated the argument advanced by James Reston and others—that the problems faced by this administration are more difficult than those encountered by earlier ones.

James Linger's examination of Soviet and European reactions to Carter's defense and foreign policies is one of the better and more useful chapters in the book. Linger depicts the sense of uncertainty and concern the president's leadership has provoked among our European allies and the Soviets, as well as European approval of his emphasis on NATO defense. The essay is, however, excessively detailed in regard to its examination of the SALT II provisions. Linger's discussion of the Carter NSC, while relevant in the context of this essay, unfortunately duplicates material examined by Korb. Another useful chapter is Doris Graber's "Intervention Policies of the Carter Administration." Graber looks carefully at traditional patterns of intervention and the restraints on such action that emerged from the Vietnam experience. She suggests that the administration has devised a new rationale for intervention—humanitarian concerns—although the durability of such a rationale is not clear. Since arms transfers have become a key form of intervention, a more careful examination of how these policies have evolved since 1976 would have been helpful.

The major shortcomings of the volume are the lack of a relevant analytical framework, an absence of continuity from one chapter to the next, and some unnecessary repetition. The volume is useful for students, but its value for foreign and defense policy specialists is limited. Two major chapters (those by Davis and Korb) appeared in substantially similar form in widely circulated journals shortly before this volume was published.

WILLIAM P. SNYDER

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Energy Future: Report of the Energy Project at The Harvard Business School. Edited by Robert Stobaugh and Daniel Yergin. (New York: Random House, 1979. Pp. x + 353. $12.95.)

The cheerful message of this extremely well-turned-out volume is that, for a few hundred billion dollars and a good dose of political leadership, we Americans can push back the menacing tide of Persian Gulf oil. The dollars are to be invested in what the editors call "unconventional" energy sources, solar power and "conservation energy," rather than in "conventional" sources like oil and gas wells or coal mines. Why we should make the financial and political effort is obvious the contributors to this volume: otherwise we and the other oil importing nations will end up paying staggeringly high sums to potentially unfriendly states like Libya, Iran, and Saudi Arabia; and, worse still, the exporting states may very well decide not to sell us enough oil anyway.

There is much in the analysis to recommend it, especially its fond attention to political variables. Because it has such an agreeable outcome, one would certainly like to believe in it. Yet there are enough minor flaws in the book, and enough sleight-of-hand tricks that work in favor of policy options the authors like and against options they dislike, that I am inclined to remain skeptical. Consider, for instance, their excellent political and institutional analysis of the obstacles to realizing the energy potential of our vast coal resources. Electric utility demand is the key to unlocking this potential, but the utilities are constrained by air-pollution regulations, regulatory restrictions on debt financing, new plant-siting difficulties, and general public suspicion of utilities' plans and motives. To open new mines in the western states requires new rail lines to bring the coal to market, but these will be slowed by the railroads' inability to raise the necessary capital, to secure right-of-way approvals, and to outmaneuver challenges to environmental impact statements. Water might be used to move coal in slurry pipes, but water is scarce in the West and the local people want to hold onto it for ranching and agriculture. The United Mine Workers are a wild card in the game, too—the coal strike of 1977–78 lasted almost four months—and the federal government has very little to say about how the card is played. These and a number of other carefully delineated problems convince Mel Horwitch, the author of the coal chapter, that coal "cannot be *the* transitional energy source," much to our "inevitable disappointment." Yet, when it comes to favored energy sources, we see no comparable hard-headedness. Although many problems are acknowledged, they all seem soluble: "Sometimes one concludes that the real challenge of energy conservation is not to do it, but rather to believe that it can be done," writes Yergin optimistically. He calls for performance rather than "specification" codes in the building industry, for example, yet entrenched labor union and local government interests have blocked this sensible reform for many years now. Utilities, he says, should be given "a major stake in retrofit," but he is too little embarrassed by his admission that Congress, as recently as 1978, enacted a ban against the utilities' moving into the retrofit business.

As for solar energy, Modesto A. Maidique first spends three pages on a fine analysis of the institutional barriers confronting the diffusion of commercially viable solar technologies, but follows these with only a breezy few paragraphs on how they might be overcome. "A model solar heating code is required," for instance, but he makes no mention of the enormous difficulties in the way of getting it widely adopted. The most important institutional change, says Maidique, would be to harness the electric utilities for marketing solar equipment. But with the exception of the California utilities, most have been resistant to the idea. "Legislation and regulation" could solve the problem, says Maidique, but without elaborating on what kind, or on how the utilities might subvert or stall such mandates during the implementation process.

An appendix to the book analyzes the shortcomings of three large-scale econometric models of the U.S. energy supply system put forward by prestigious academics and other experts during the mid-1970s. Most of the criticism has to do with the apparently unrealistic assumptions made by the model builders. This criticism is irrelevant, however, for all models—formal or intuitive, machine-processed or mental—use unrealistic assumptions, owing to the need to clarify the logic and simplify calculations. The modelers were not wrong to exclude "national politics" and the like from the models, even though the exclusion did lead to quite erroneous predictions. Their mistake, as the appendix authors cursorily imply, was to refrain from testing the sensitivity of the models to changes in assumed political parameters. Yet this is precisely the error in the political process model that underlies I. C. Bupp's main argument against further reliance on nuclear energy. Anti-nuclear critics have managed to produce a policy "stalemate" that cannot be ended easily, he says. "An increasingly representative society guarantees the nuclear critics a base of political power, which means that resolution of the nuclear safety controversy will require more than a consensus of established scientific engineering judgment," he writes. Bupp himself does a lot of artful fence-straddling on the controversial questions of nuclear safety, cost-effectiveness, and the technical feasibility of safe waste disposal, and in the end he offers no reason other than the political power of the anti-nuclear movement to give up on nuclear energy. However, one wonders if a change in the parameters of Bupp's political model—say, a determined and outspoken president, backed by Congress and a solid bloc of state and local political leaders—could change the situation. Perhaps not, but failure to pose the question simply imitates the errors of the aptly criticized econometricians.

Neglect of the leadership variable in the cases of coal and nuclear energy, which the editors do not care for, and immense overconfidence in the power of leadership with regard to conservation and solar energy, which they do care for, should signify to alert readers that the book is not as analytically weighty as its many favorable reviewers seem to think. The editors' main conclusions could still be right, of course, but it is a pity that this most promising political analysis of energy policy to date did not guard more carefully the line between positive and normative theorizing.

EUGENE BARDACH

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City and Suburb: The Political Fragmentation of Metropolitan America, 1850–1970. By Jon C. Teaford. (Baltimore and London: Johns Hopkins University Press, 1979. Pp. vii + 231. $15.00.)

This is an historical account of the political balkanization of metropolitan America organized conceptually around two competing value sets which have worked alternately to promote governmental fragmentation on the one hand and integration on the other. Jon Teaford casts the value of local suburban autonomy in its many legal, political, and social manifestations against the desire for improved public services, economy and efficiency considerations, and American values which hold growth and "bigness" as desirable attributes of the urban place. Both value sets have operated to shape metropolitan America and, while the forces of fragmentation have prevailed, such was not always the case. In this context, the author examines a variety of attempts, both successful and unsuccessful, to achieve political integration via annexation and consolidation.

City and Suburb is an ambitious undertaking which is carried off rather well. Even though the scope is national, the time span covers 120 years, and the volume contains but 186 pages of text, Teaford has done a thoroughly competent job of extracting the principal threads to weave his history of metropolitan area fragmentation. Moreover, he provides a surprising amount of detail and numerous examples drawn from virtually every major metropolitan area in the nation. Thus, while the casual reader may be mildly bothered by the author's leaps around the metropolitan landscape, the breadth of research and coverage clearly enhances the quality of the book. Finally, the text is liberally spiced with quotations from both primary and secondary sources which provide instructive and sometimes amusing insights into the ultimate city-suburban destiny.

Perhaps because Teaford is an historian, the most outstanding attribute of his work lies in the deft handling of events between 1850 and 1930 culminating with attempts to realize political integration in the St. Louis, Cleveland, and Pittsburgh metropolitan areas. The concluding chapter, "The Continuing Crisis," focuses on the post-1930 period and is something of a disappointment. Utilizing a scant 15 pages of text, this chapter barely transcends the level of an epilogue and, although some reference is made to a few contemporary sources, the author's forte is clearly history.

One disconcerting feature of this book is a superficial excursion into "The British Alternative" (Ch. 4) to metropolitan fragmentation which, of course, was never really an "alternative" in the United States. In any case, the reader is suddenly immersed in the Local Government Act of 1888, the role of the Local Government Board, centralized control of local government, and the consolidation of Birmingham, England in 1911. The stated rationale for this excursion is "contrast," but it adds little or nothing to understanding metropolitan fragmentation in the United States. At best, it is a hindsight argument to the effect that if nineteenth-century American state legislatures had only behaved like the British Parliament, the present metropolitan mess might have been avoided. For some inexplicable reason, the vast gulf between British political culture and institutions and that of a nineteenth-century American state is simply ignored. Fortunately, the British connection is soon terminated except for some passing references to the London County Council.

It is unlikely that the urban scholar will discover anything new or startling in this book. At the same time, it is a sound piece of work that is well written, well researched, and endowed with copious notes and an excellent bibliography. And because it is a lucid account of the why and how of metropolitan area fragmentation, it is a valuable addition to the literature.

DAVID G. TEMPLE

West Virginia University

The Changing Structure of the City: What Happened to the Urban Crisis. Edited by Gary A. Tobin. (Beverly Hills, Calif.: Sage Publications, 1979. Pp. 320. $18.50, cloth; $8.95, paper.)

This book grew out of a symposium at which most of the participants were academics; however, a few practitioners, Mayors Kenneth Gibson of Newark and Tom Bradley of Los Angeles, and Senator Charles Mathias of Maryland also made contributions. Gary A. Tobin provides an introductory essay designed to give coherence to the collective enterprise.

The main threats tying together these disparate essays are the following: concern with the urban crisis of a decade or so ago, whether or not the conditions for renewed urban violence still exist, and finally, whether or not the potential for disorder can be defused. Tobin, and others, assert that the essential ingredients of the urban crisis are still with us. The pathologies of the city, crime, poverty, race, poor housing, fiscal insolvency, poor management and the lack of a coherent national urban policy continue, and will continue because "no major change [has been made] in the structure of the economic and social system" (p. 10). Thus while disruptions have been halted, the bases for the urban crisis still exist. "Until we seriously address the basic issue of widespread poverty among the minorities who are concentrated in our urban centers, the potential for another period of violence, disruption and disorder in our cities remains very strong" (p. 17).

More than this, as Tobin notes, the nation, faced with high inflation and rising costs of government, seems to be turning away from social reform. Social programs are considered expensive and wasteful. Tax revolts seem to be the order of the day. Even the capacity of government to act is being challenged. The current mind-set of the nation seems to reflect the belief that, given the strains on our fiscal resources, we are doing about as much as can be expected. Even more, perhaps, governmental policy seems to assume that the crisis will go away if we ignore it.

Compounding the pathologies of the city and the attitude of the nation is an institutional inadequacy noted by Tobin. Cities are creatures of the state, limited both in their powers and scope of their activity. Purely local issues, he asserts, can be dealt with through effective or improved management at the local level. But national problems centered in our cities, the ingredients of the urban crisis noted above, cannot be solved by local units simply because their resources are so limited. Therefore, if the city is to act in connection with national problems *in* the city, it must assume the role of an advocate. However, in the long run "the national government must provide the resources, leadership and planning to combat problems connected with race and poverty in the cities" (pp. 16, 71).

Fleshing out the general theme are a number of articles that deserve mention. Thomas Galloway raises serious questions about the capacity of government to act, while Michael Parkman's study of school desegregation asks whether the government is willing to act. Dennis Judd and Margaret Collins present an insightful analysis of the basic theory of urban revitalization, "enticing business and the middle class to locate in the central cities" (p. 177). Dudley Burton's "Energy and the Urban Form" is particularly timely because it relates the costs of energy to transportation technology and the ultimate shape and form of the city.

Given the book's general level of excellence, one hesitates to take issue with some of the points that it makes. Yet one must do so in the interest of clarification. Thus while it is obvious that many of our cities are still experiencing difficulty, the belief that the potential exists for another round of violence and disruption in our cities must be questioned. Like our earlier experience with the Great Depression of 1929–1933, we have learned a great deal about the management of urban crisis, especially since the long, hot summers of the sixties. Anticyclical grants, CETA and its predecessors, general revenue sharing, and such remnants of the Great Society programs as community development, urban action development grants, affirmative action, increased decentralization of community decision making–to mention but a few of the major efforts to help our cities–may not solve the urban crisis, but at least they have taken the edge off the despair and anger of those who are forced to continue to reside in our urban ghettos.

Furthermore, the pat scheme for the remedy of our urban ills suggested by the book, namely, a national urban policy to give direction and coordination to the federal effort, may not be as essential as Tobin and other seem to indicate. No doubt coherence is needed, direction is needed, but we have no evidence demonstrating that if these ingredients were supplied, our cities would be better off than they are today with our existing and perhaps scatterated approaches. But even if the nostrum of a national urban policy is accepted, one must wonder why neither Congress nor the president has adopted the idea, even though at times both have given lip service to it. Obviously, political realities have made national goal-making a hazardous enterprise. Since it is not attainable, one must accept the reality and work within its framework.

Finally, while the book stresses the urban crisis, one must ask: whose crisis is at issue? Is it one that concerns large cities, small cities or all cities? The book's generalizations seem to be derived from the problems of large urban areas, particularly the older cities of the Northeast and the Midwest. But the problems of these so-called frostbelt cities are quite different from those of the sunbelt where growth seems to be the order of the day. Again, do central cities with a population of under 500,000 have the same difficulties as those that are larger in population size? Is the age of a city, as well as its size and pattern of growth, related to the urban crisis? If so, then the nature of the "crisis" may (must?) vary according to the city's characteristics, and, therefore, "the potential for another period of violence" may be less for some cities and more for others. Thus the tendency to equate the ills of our large metropolitan centers with those of all our cities may be misleading.

Despite these few caveats, however, I can recommend the book highly. There is much material that is extremely useful in it, the essays have a high level of excellence and, on balance, the book is a careful and sober assessment of the present status of the central city.

BENJAMIN BAKER

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The Social Gamble: Determining Acceptable Levels of Air Quality. By Richard J. Tobin. (Lexington, Mass.: Lexington Books, D.C. Heath, 1979. Pp. xvi + 174. $17.00.)

At first glance, Richard Tobin's *The Social Gamble* appears to be just another implementation study. It is an analysis of the Public Health Service and Environmental Protection Agency's attempts to implement and enforce air quality standards pertaining to sulfur dioxide emissions. The focus here is upon stationary pollution sources (i.e., power plants, steel mills, etc.), as opposed to automobile emissions. But the work is more than that. It is an investigation of a particularly troublesome political problem: how does our government determine the acceptable level of risk to the public from the activities of a few, and then impose regulatory restrictions on them to protect the public's welfare? More specifically, how does the government determine clean air standards without the benefit of adequate scientific data, and then impose pollution standards upon politically powerful industries? The answer given here is: "not very well."

The author's conceptual approach to structuring his analysis is interesting, in that he begins with a discussion of the literature on public decision making which pertains to the determination of "acceptable risk," and then considers important concepts in the growing body of research on regulation and implementation.

While risk assessment frames Tobin's analysis of the legislative history of developing clean air standards, he uses concepts based on regulation and implementation research to analyze the administrative behavior of the Public Health Service and the EPA in implementing these standards. It is a study rich in insight of inter-agency relations, congressional oversight, and presidential leadership in environmental policy making. The study is classic "muddling through." The EPA appears to be the embattled regulatory agency which, in its pursuit of the public interest, was hamstrung by the Congress, maligned by the states, ignored by presidents, abused by the OMB, and openly attacked by private industry. Unfortunately for the EPA, the Clean Air Act Amendments of 1970, passed during the peak of the environmental "issue attention cycle," had to be implemented during our first energy crisis. Apparently many people, including several presidents, have been willing to sacrifice a measure of the public's health in order to perpetuate the myth of cheap energy.

Moreover, Tobin makes another important point. The type of regulatory process initiated by the EPA is substantially different from the regulatory practices traditionally analyzed by political scientists. Traditional regulation has been interpreted as market intervention, or the substitution of administrative for market decision making. Contemporary environmental regulation can be viewed as what the author calls "social regulation," because the objective is to alter individual and corporate behavior. A kind of large-scale behavior modification, if you will. As Lynton Caldwell noted early on, our democratic institutions and ideologies are poorly suited to assume this task. A weakness in this analysis, however, is the author's omission of important works on environmental regulation and implementation by Sabatier, Thomas, Ball, and others. Tobin merely uses Bernstein's cooptation theory, which, as Sabatier pointed out, has shortcomings when applied to contemporary regulatory situations.

However, the book is a useful case study of environmental standard setting and enforcement decision making and will probably become "the work" on sulfur dioxide pollution policy. It illuminates the political, administrative, and scientific problems inherent to environmental regulation, the difficulties the federal government has in making tradeoffs between environmental and economic values. In the years to come, this type of decision making will become more and more salient (but just as difficult) as we as a nation enter an era of natural resource scarcity. It is on this basis that this book is recommended.

TERRY D. EDGMON

North Carolina State University

Politics and Planning: A National Study of American Planners. By Michael Lee Vasu. (Chapel Hill: University of North Carolina Press, 1979. Pp. xviii + 235. $17.00.)

In *Politics and Planning* Michael Vasu states his objective is "to provide a profile of planners with respect to their perception of both the planning function and their role in it" (p. 6). Planning, Vasu argues, is essentially a political process that authoritatively allocates values (p. 12). Despite the political importance of plans, the author feels that planners can exercise a great deal of discretion. Planning recommendations are based on the interaction of the planner's personal values, the values of the planning profession, the planner's expertise, and the situation. As a result, Vasu argues, a study of the opinions and beliefs of planners is an essential undertaking.

Politics and Planning is a report of a mail survey of 775 randomly selected members of the American Institute of Planners, the major professional organization. The two major types of questions concern planners' attitudes about planning and planners' general political attitudes. The latter attitudes are compared to responses from a national survey of the American people. The two dominant hypotheses are that planners' values are affected by their formal training as planners (an indicator of professional socialization) and employment by public versus private organizations (a situational variable).

When examining planners' views of their own profession, Vasu contrasts the data to classic planning values (environmental determinism, utopian vision, and political reformism). Vasu finds substantial variation among planners, with many of the planners rejecting classic values and endorsing those advocated by the advocacy-planning movement. A majority of planners accept the position that plans are not neutral. Despite this view, a majority also believe planning is a profession, that a theory of planning exists, and that a national planning agency is a good idea. Neither formal planning training nor public/private employment affects these attitudes.

Planners, as Vasu predicts, do differ significantly from the American people. They are more liberal, more likely to be Democrats, and more likely to participate in political activities. These differences remain even with controls for education, income, or social status.

When their views on traditional reformist issues are probed, planners are fairly divided. Little consensus exists on whether or not there is a solitary public interest, whether or not planners are good judges of the public interest, and whether or not citizen participation is representative. They do, however, generally endorse structural reforms for city government (e.g., city manager government, nonpartisan elections).

Vasu, in general, has provided a valuable book to students of professionalization. He has provided a solid data base on the beliefs and attitudes of planners. We need several similar studies to reveal the values of economists, lawyers, natural scientists, policy analysts, etc. so that scholars can probe the relationship of professional values to public policy.

KENNETH J. MEIER

University of Oklahoma

Speaking Truth to Power: The Art and Craft of Policy Analysis. By Aaron Wildavsky. (Boston: Little, Brown, 1979. Pp. xiv + 431. $13.95.)

Aaron Wildavsky's latest book is about a career, his career as a teacher and practitioner of policy analysis. It is in essence an edited anthology with a single unifying purpose—to develop his view of the meaning and role of policy analysis. As it turns out, policy analysis cannot be given a single crisp definition. It is, says Wildavsky, "an applied subfield whose content cannot be determined by disciplinary boundaries but by whatever appears appropriate to the circumstances of the time and the nature of the problem." As the subtitle of the book makes clear, it is an art and craft, rather than a discipline.

Other writers have made this same point about the elusive nature of policy analysis. There does, however, seem to be a way out of this dilemma, and it is suggested in the very first paragraph of Wildavsky's book. "I think of myself," he says, "as a political economist." Indeed, one might simply define policy analysis as applied political economy. Wildavsky's treatment of the subject, including the 12-page appendix on "Principles for a Graduate School of Public Policy," suggests that he might have made an even stronger case for blending the concepts and techniques of economic analysis with a thoroughgoing understanding of the institutional setting and politics of the problem area under analysis.

For educational institutions, such as the Graduate School of Public Policy at Berkeley, that seek to impart this essential blend of economics and political science, Wildavsky's book should be a basic text. It is a gold mine of insightful and provocative writing about the role of policy analysts, the kind of subjects they deal with, and the way they should practice their craft. The book deserves a special commendation for being highly readable, even fun to read. Wildavsky may make you angry, but he will never make you sleepy.

Wildavsky being a charter incrementalist and an outspoken pluralist, it should be no surprise that both sets of ideas are strong thematic currents of his latest book. He describes the policy analyst's role as one that should respect markets and federalism and recognize that policy analysis involves continuous accommodation and adjustment. Policy, says Wildavsky, should be thought of as a "temporary resting place," not a "permanent cure."

For those who want some examples of policy analysis in action, Wildavsky provides four chapters discussing specific policy problems. Their titles alone are worth the price of admission—as, for example, "Doing Better and Feeling Worse: The Political Pathology of Health Policy." My favorite chapter in this group is the chapter on education, "Learning from Education: If We're Still Stuck with the Problems, Maybe We're Taking the Wrong Exam." The other two chapters in this section are on alternatives to the tax deduction for charitable contributions and the distribution of urban services.

There is one issue which Wildavsky's book wrestles with but never quite pins down—namely, the issue of whether the analyst should be an advocate, and, if so, to what degree. At one point he is quite explicit. "I have never been sympathetic to the view that facts and values, except as intellectual constructs, either are or ought to be kept separate." It is proper—even necessary—for the analyst to be "persuasive," says Wildavsky. And yet at other points, he seems to back away from urging an advocacy role for policy analysts, referring, for example, to "the danger that interest will overwhelm intellect." Continuing, he states, "My conclusion is that policy analysis makes more sense as an aid to (rather than substitute for) the politics of social interaction." The book could be clearer on this point, perhaps helping graduates of Berkeley and other public policy schools to sort out the conditions, although they are never very clear, where one is employed primarily as an expert "on tap," as opposed to positions where his or her role is essentially that of a policy maker "on top." The reader may not find a satisfactory resolution of this problem, but there are soaring discussions of many other problems that come to conclusions in a firm, clear, useful manner, making this book an essential library acquisition for policy craftspeople, a kind of *Joy of Cooking* for domestic policy.

RICHARD P. NATHAN

Princeton University

The Declining Significance of Race: Blacks and Changing American Institutions. By William Julius Wilson. (Chicago: University of Chicago Press, 1978. Pp. xii + 204. $12.50.)

This is an incisive and impressive analysis of the role of race and class in American society and their impact on race relations. The author finds that there have been three major historic stages of black-white contact in the United States: the antebellum era of slavery, characterized by the plantation economy and racial-caste oppression; the period between the Civil War and the New Deal era, characterized by industrial expansion, class conflict, and racial oppression; and the post-World War II era, marked by progressive transition from racial inequalities to class inequalities.

Wilson believes that in the present, modern industrial stage, there have been fundamental changes in traditional patterns of race relations—especially in the labor market. Consequently, the opportunities, success, and upward mobility of individual blacks are determined now much more by economic class position than by race. Thus "a preoccupation with racial conflict," he says, "obscures fundamental problems that derive from the intersection of class with race" (p. ix). He argues that the basis of racial strife in the industrial sector has been eroded by economic and political changes calculated "to mediate or resolve class and racial conflicts grounded in the social relations of production" (p. 88).

Specifically, the economic changes include the decentralization of business, shifts from goods-producing to service-producing industries, expansion of corporate and government sectors, rise of a segmented labor market, entrance of black workers into more liberal labor unions, increasing technology and automation. Political changes include enactment of laws protecting unions and the establishment of affirmative action programs. These developments, he concludes, have led to the emergence of an essentially bifurcated black class structure: a burgeoning middle class of talented, educated blacks who benefit from the expanding corporate and government sectors, and a large black under-class increasingly concentrated in unskilled, low-paying, dead-end jobs.

Expansion of the government and corporate sectors has created many upper-level jobs for qualified black and white workers, and this has reduced job competition markedly and minimized racial friction. While lack of white competition for the low-paying jobs has resulted in little job-regulated strife between lower-class black and white workers, Wilson does not claim that racial conflict in the United States has ended, but rather than the basis of conflict has shifted from the economic—job-related—sector to the sociopolitical sector—over housing, control of community schools, recreational facilities and, at times, of city government. These are areas of conflict between those blacks and whites who have inherited the central city after extensive suburban flight.

Assessing this development, he explains that "A new set of obstacles has emerged from basic structural shifts in the economy. These obstacles are therefore impersonal but may prove to be even more formidable for certain segments of the black population" (p. 1)—the one-third of blacks who form the under-class.

While Wilson recognizes that sharp class divisions between blacks exist, and appear to be deepening, he does not speculate on the future consequences of what has been called a "schism" among black Americans. He points out that the economic gap between the black poor and more affluent blacks led to a shift, after the urban riots of the late 1960s, from the middle-class oriented civil rights movement of the early sixties to a concern for ending the cycle of poverty, unemployment, and inadequate education. The politicalization of lower-class blacks led to a "new mobilization of political power" (p. 88) which, in turn, forced middle-class black politicians to address more vigorously the problems of their constituencies—the ghettos.

This well-written volume makes a solid contribution to the proliferating literature on race and class in the black experience. It is a brilliant distillation of persuasive reasoning from a wide array of sources and deserves careful examination by serious students. Wilson's scholarly presentation makes clear the factual existence of the black middle class and a depressed but increasingly politicized black under-class. The questions are: how the latter will perceive and react to its deteriorating position; whether the former will recognize and act upon a sense of racial responsibility; and whether the emerging feeling of black awareness, social consciousness, and racial pride in both groups are strong enough to bridge the class gap and provide guidance in addressing the insistent problems of all black Americans.

ROBERT E. MARTIN

Howard University

Shattered Peace: The Origins of the Cold War and the National Security State. By Daniel Yergin. (Boston: Houghton Mifflin Company, 1977. Pp. x + 526. $15.00, cloth; $5.95, paper.)

In the vast library of books on American foreign policy, the largest and richest body of literature is devoted to the origins of the cold war. Authors have sought to allocate blame to the Russians, or to the Americans, or to both, or to chance or fate. At a deeper level, they have sought for explanations in the nature of a bipolar international system, in the ideologies of liberalism and communism, in the dynamics of democratic and totalitarian political systems, in the expansionism of the American capitalist economy, in bureaucratic interests, or in the personal background and individual psychology of leading policy makers. Today, nearly two generations after the beginning of the cold war, there is still no definitive interpretation of its origins, nor is there likely to be one. Rather, the continuing and perennial ebb and flow in Soviet-American relations, the oscillation between periods of accommodation and confrontation, of detente and renewed cold war, will be reflected in a continuing ebb and flow among the various and conflicting explanations of what happened long ago in the 1940s.

Daniel Yergin's *Shattered Peace,* written with grace, lucidity, and wit, contains a little of each of these explanations. Some readers will thus see it as a wise and balanced integration of them; others will see it as an eclectic mishmash, a good story but a poor theory.

Yergin believes that it was inevitable that there would be some degree of confrontation between the United States and the Soviet Union after the Second World War, but that it was not inevitable that there would be a cold war, with the resulting militarization of American foreign policy and the rise of what he calls "the national security state." Rather, for Yergin, the origins of the cold war lie in a particular set of ideas about the Soviet Union, which were adopted by American leaders in 1945–1946 and which saw Soviet foreign policy to be propelled toward consistent expansionism by communist ideology and totalitarian politics. Yergin calls this set of ideas, "the Riga axioms" (after the capital of Latvia, from which a small corps of American foreign officers like George Kennan observed the Soviet Union before the U.S. recognized it in 1933). He contrasts this set with another, "the Yalta axioms," which were held by Roosevelt and a few advisors and which saw the Soviet Union as being a great power rather like other great powers, a nation which would have important differences and disagreements with the U.S. but one with which the U.S. could negotiate. The death of Roosevelt in April 1945 suddenly removed the main support for the Yalta axioms, and Truman, inexperienced and unknowledgeable in foreign affairs, relied heavily upon the advice of the traditional foreign policy bureaucracy, which adhered to the Riga axioms. U.S. policy makers thus turned toward confrontation rather than negotiation with the Soviet Union (confrontations accentuated by

Stalin's own Marxian axioms about the U.S.). These confrontations in turn led to the development of what Yergin calls the two "commanding ideas" of anti-communism and national security and then to the policy of containment and to the rise of the military and the decline of diplomacy in the conduct of U.S. foreign policy. By 1948, Europe was divided and polarized, the cold war was institutionalized, and there was no turning back.

Why did U.S. policy makers accept and apply the Riga axioms so readily? Yergin focuses on the personal background of government officials and on particular bureaucratic interests. Among the major strengths of the book are the always interesting and often elegant and amusing portraits of the major figures of the time. Insofar as the book has a theoretical perspective, it is closest to the psychological and the bureaucratic politics models of American foreign policy.

Conversely, the book does not say very much about the role of American electoral politics or of economic interests. Yergin does discuss, however, the role of right-wing Republicans in promoting anti-communism and the role of the American aerospace industry in bringing about the national security state.

Like most psychological and bureaucratic interpretations, Yergin emphasizes misperceptions, accidents, confusion, and inadvertence among policy makers. He is at pains to deny that either the Americans or the Russians planned particular outcomes (although some of his evidence could be easily used to argue that they did). And he carries this line of thought a step further. He believes that when leaders become convinced that the other side is committed to a plan of expansion, this conviction eliminates the possibility of diplomacy.

One of the most compelling and poignant sections of the book deals with U.S. policy toward Czechoslovakia in the year before the Communist coup of February 1948. Yergin is persuasive in arguing that a more subtle and supple U.S. diplomacy could have preserved substantial liberties and democracy in Czechoslovakia, but that U.S. policy makers had already written the country off to the Soviets. In short, the Czechs and Slovaks were in large measure victims of an American self-fulfilling prophecy.

Most political scientists will find Yergin's interpretation of the origins of the cold war congenial if not very original. Its theoretical implications are pluralist rather than statist (belief in the state as a unitary rational actor) or Marxist. Its political implications are liberal (the mistakes and misperceptions of particular policy makers can be corrected the next time around with new policy makers) rather than conservative (we must increase our defenses against Soviet expansionism) or radical (we must alter something fundamental in American society). And, indeed, when Yergin was writing in the mid-1970s, the Riga axioms seemed to have been largely abandoned among policy makers, and detente or the Yalta axioms seemed to have been widely accepted.

In 1980, however, there seems to have been a second coming of the Riga axioms among American leaders. How can this be explained? Perhaps a few years hence, another young historian will write a book about the origins of the Second Cold War, titled *Shattered Detente*, which will explain the sad turn of events by the sudden departure of the main supporter of detente, Henry Kissinger, and by the fact that Jimmy Carter, neither experienced nor knowledgeable in foreign affairs, relied heavily upon the advice of his national security advisor, Zbigniew Brzezinski, whose anti-Sovietism was accentuated by his Polish background, and upon the advice of the traditional foreign policy bureaucracy, which adhered to the old cold-war conventional wisdom. For there will always be people who will want to find the origins of their own particular time of troubles in accidents and chance, in the mistakes and misperceptions of well-intentioned persons, rather than in some deeper and more fundamental cause which seems to recur again and again.

JAMES R. KURTH

Swarthmore College

Comparative Politics

Communism and Political Systems in Western Europe. Edited by David E. Albright. (Boulder, Colo.: Westview Press, 1979. Pp. xx + 379. $21.00.)

This compendium of recent studies of Western European Communist parties was compiled by David E. Albright, editor of *Problems of Communism.* It is a useful book (especially since most of the information it contains is quite current), although most of its chapters are descriptive rather than analytical and, as in all edited volumes, their quality is somewhat uneven.

In an introductory chapter, Albright presents a comprehensive survey of the present status of over two dozen Communist parties in Western Europe, ranging in size from the Italian Communist party to seemingly insignificant Maoist splinter groups. He systematically assesses them according to their "images" or self-identifications, their basic strategies and their "opportunities" (i.e., the socioeconomic and political contexts within which they operate). This chapter conveys the diversity of these parties, and (like the other chapters in this book, but unlike many of those in the recent volume on Italian Eurocommunism edited by Austin Ranney and Giovanni Sartori) avoids cold-war polemics. Albright concludes that "the mix of opportunities, self-identifications and strategy differs so much from Communist party to Communist party in Western Europe that the role of each party in its individual political system is to a substantial degree unique" (p. 29). The most important of these distinctions separates the "autonomist, revisionist" parties (those of Italy, France, San Marino, Spain, Great Britain and Sweden) from "orthodox communist parties" (a category which includes the parties of Austria, Belgium, Denmark, West Germany, Ireland, Luxembourg, Portugal, Malta and Switzerland). Given the scope of this survey, it is unavoidably somewhat superficial, but it is valuable, nonetheless.

The next eight chapters present more in-depth studies of several of these parties—those of Italy, France, Spain, Portugal, Greece, Finland, Norway, Sweden, Denmark, Iceland and Great Britain—and the book concludes with a study of the doctrinal disputes which erupted at the 1976 European Conference of Communist Parties. The best of these chapters were written by Trond Gilberg and Giacomo Sani.

Gilberg's well-written piece is a comparative study of Nordic communist parties. One of them (that of Iceland) is among the most successful of West European communist parties, while the principal communist parties of Denmark, Norway and Sweden have met with consistently lower levels of electoral support or have declined precipitously in recent years. Among those factors which Gilberg regards as most important in explaining these differences are a party's ability to avoid internal schisms, and to seize upon popular issues and create for itself a national (if not nationalist) identity separate from (indeed, as distant as possible from) that of the CPSU.

Giacomo Sani's fine chapter is a comprehensive but succinct assessment of the substantially enhanced role in Italian politics played by the PCI since 1975. He summarizes the principal features of Berlinguer's "historic compromise" strategy, attempts to explain the steady increase in electoral support for the party in the mid-1970s and its more prominent position within Italian government institutions, and presents an initial assessment of the consequences of that more active governmental role. The PCI's recent steps out of "the ghetto of permanent opposition," he argues, can be explained as a product of several social and political changes: (1) the more moderate public stands taken by the PCI elite; (2) the increased volume and neutrality of media coverage of the party; (3) a decline in hostility towards the PCI on the part of non-communist political elites; (4) substantial demographic turnover which has progressively reduced the proportion of the electorate holding anti-communist attitudes; (5) a complex of social changes (including the secularization of Italian society (which have tended to "break down the ties between individuals and their traditional political subcultures, rendering them open to new political appeals" (p. 62); and (6) the image of administrative and governmental efficiency which has resulted from the party's long-term experiences in local government bodies. The consequences of the PCI's increased role include the party's growing legitimacy as a democratic institution, and an increase in its prestige, access to patronage and influence over legislation.

The remaining chapters are almost exclusively descriptive and, largely for that reason, not wholly satisfying. A discussion of the two contributions by Eusebio Mujal-Leon is illustrative. Mujal's chapters are accounts of the behavior of the Spanish and Portuguese Communist parties during the recent transitions from authoritarian to democratic regimes. Given the paucity of published English-language studies of contemporary Iberian politics, they constitute a valuable contribution to this growing field. Both studies, however, focus almost exclusively upon the short-term tactical maneuvers of the PCE and PCP. Mujal does not attempt a systematic explanation of why these parties have behaved so differently in recent years—with the PCP heavily relying upon the "dynamics of force" in its attempts to seize power, while the PCE has played a constructive role in founding and stabilizing a new democratic regime—but he implies that these tactical differences were merely responses to the different political contexts of the Spanish and Portuguese transitions. It would have been preferable if Mujal had more rigorously examined this contention and weighed it against alternative factors, such as the basic ideological stands of the two parties. Instead, one comes away from these useful but entirely descriptive chronologies with little appreciation of the significant differences between these two Iberian Communist parties.

RICHARD GUNTHER

Ohio State University

Comparing Public Policies: New Concepts and Methods. Edited by Douglas E. Ashford. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 256. $17.50, cloth; $7.50, paper.)

The essays in this, the fourth volume in a "Politics and Public Policy" series published by Sage, reflect both the eclecticism and the provocativeness which currently characterize the comparative study of public policy. This eclecticism is to be expected. The use of comparison in political science has always involved disagreement and diversity; the study of policy, even when not conducted comparatively, exhibits much of the same diversity. The essays thus reflect the two traditions they represent. The essays are also provocative. Readers should not approach the book hoping to get an overall view of what comparative policy studies are up to or should be up to. They *can* get from it some idea of the approaches of prominent and thoughtful analysts regarding what should be compared and how.

As already implied, the essays cover an extremely broad range. To give the book some focus, the editor has sensibly grouped the essays around four major themes—the logic of comparison, institutions and comparison, economic theory and comparison, and, finally, the concept of policy as the determinant of politics. These themes give an idea of the different angles from which the authors approach the comparison of public policies. Even within each category, however, the essays are disparate. Nonetheless, most of the essays are stimulating enough to offset with originality this lack of continuity.

One major question asked by several authors concerns which variables or dimensions should be examined in comparing policies. Ashford, interested in "how states pursue similar objectives by very different means" (p. 90), argues that the study of institutional arrangements surrounding policy will give us insights into the nature of the modern democratic state. Heisler and Peters emphasize the importance of including the dimensions of time and multiplicity in levels of government in the comparison of policy. Lowi, beginning from the perspective that "politics cause politics," considers the impact of policies upon bureaucratic structures, calling for a "fusion of public law, public administration, and political behavior" in the comparative study of public policies. Friedland, Piven, and Alford convincingly argue that studying the relationship between institutions concerned with economic growth, institutions concerned with political integration, and popular political participation gives us important insights into the nature of the modern capitalist state at various stages of evolution.

Charles Anderson asks different questions. In fact, he raises a cluster of difficult issues in what is probably the most trenchant essay in the volume. The basis of Anderson's argument is that policy is intrinsically normative. He therefore argues that "a normative element" is "at the very heart of any effort to develop a systematic, comparative study of public policy" (p. 20). He further points out that "normative analysis has an analytic rigor of its own"—a rigor which is not known, or learned by, most empirically oriented policy analysts (p. 21). Such analysts consequently use normative evaluation with a casualness and crudity which they would never accept in the empirical aspects of their work.

Anderson therefore argues that policy analysts must be much more precise in defining exactly what they are doing:

> My plea is not that students of comparative policy eschew social criticism, but that we clearly distinguish this question from assessment of the performance of a polity or government. It is one thing to render a judgment on a regime on the grounds that it failed to do what it set out to do, and quite another to rate it against some universal norm of political

> achievement. The first strikes me as closer to the aim of an empirically based, comparative study of public policy. The second seems to derive from the activity of normative political theory. Both, I am arguing, are appropriate to the agenda of comparative policy studies as long as we are clear about their implications (p. 30).

Anderson here makes a point to which most political scientists are not sensitive: normative analysis has its own "canons of procedure and standards of enquiry." Good policy analysis should recognize this fact, especially when such analysis is consciously evaluative.

Since Anderson's essay is both complex and important, it is impossible to do it justice here. There is much I would argue with Anderson about; but his is a bold attempt to make policy analysts ask themselves questions which they would generally rather avoid. For there is no question that their enterprise would become both more complex and more disciplined—as well as intellectually more demanding—if Anderson were heeded.

In sum, the contributors to this volume argue for various assumptions, methodologies, models, units of analysis, and steps toward theory formation. They also, however, lack precision. The term "policy" is used in an almost casual way by nearly all the authors (Lowi is the major exception). Contributors seem to assume that political scientists know what they mean when they write about "policy" and that such agreement is based upon explicit analysis (or at least definition) rather than intuition. I would argue with both assumptions, for the term "policy" is often used very loosely in studies focusing on the United States. To use it in the same way in a comparative context seems certain to multiply the ambiguities geometrically. This volume does not help us in thinking about how to avoid that particular pitfall. In other ways, however, the authors do stimulate our thinking in ways that are likely to be productive for the comparative study of public "policy."

ALBERTA SBRAGIA

University of Pittsburgh

Political Attitudes in Venezuela: Societal Cleavages and Political Opinion. By Enrique A. Baloyra and John D. Martz. (Austin: University of Texas Press, 1979. Pp. xix + 300. $19.95.)

Political Attitudes in Venezuela: Societal Cleavages and Political Opinion develops an exhaustive portrait of the Venezuelan electorate of 1973. In their effort to write a preliminary statement relating a society's major social and cultural cleavages to its mass political attitudes, Enrique Baloyra and John Martz present a most thorough and provocative review of the literature on social class, cultural pluralism, ideology and partisanship. This 1979 volume can be considered a sequel or companion to a 1976 publication by the same authors entitled, *Electoral Mobilization and Public Opinion: The Venezuelan Campaign of 1973* (North Carolina, 1976). In their 1976 book, Baloyra and Martz probed the significance of the electoral process in Venezuela, focusing on the behavior of the political parties as they competed for popular support in the 1973 campaign.

This pioneering work applies quantitative social science methodology to the study of political attitudes in one of the Third World's few functioning democracies. The authors' analyses of the results of a fall 1973 survey of over 1,500 adult Venezuelans forms the core of the book. Fortunately, the questionnaire used in this survey was reproduced in its entirety along with the response frequency of each item. This enables readers to draw their own conclusions regarding the raw data of the study.

With regard to their analyses of the social-class context of political opinion and behavior, the authors found little support in the Venezuelan data for the notion of working-class authoritarianism or of a supposed working-class and low-status group indifference to the fate of democratic institutions. Those individuals identified in the survey as "working-class" tended to express higher levels of support for the rules of the democratic game than did their middle-class compatriots. Given the existence of compulsory suffrage and the "populist" nature of Venezuela's political parties, "poor" and "working-class" participation in elections equaled that of high-status individuals. Perhaps, due to their greater "dependence on the Patron state" (p. 54) for their welfare, the "poor" also tended to be more active in the political parties than were the high-status groups. However, these loyal, low-status partisan activists expressed less personal political efficacy and more discontent with the performance of "their" democracy than their "less loyal" middle-class compatriots.

In their analysis of the cultural dimension, Baloyra and Martz found regionalism to be a declining factor in public opinion, although they found a significant regional variation in the distribution of practicing Catholics in Venezuela. Their chapter on the "ideological connection" was most stimulating although less convincing than those on social class and cultural pluralism. The difficulty lies in the nature of the beast they tried to tame. For example, in Venezuela, individual ideological leanings apparently are not a critical factor in determining party affiliation or voting. The militants in Venezuela's two clearly identifiable left-wing parties were an exception to the rule, but these two parties received a bare 10 percent of the legislative vote that year compared to the 75 percent received by the two principal "populist" parties, Democratic Action (AD) and the Social Christian (COPEI). The authors also acknowledge, citing a comparable Chilean study, that distinct elements of a public's belief system may not necessarily hang together. "Shifts in the preferences of the Chilean electorate toward leftist parties were not accompanied by increases in the . . . left orientation of Chilean public opinion" (p. 122). In addition, while "communism" was overwhelmingly rejected by the 1973 sample, primarily because of its association with the Cuban-inspired violence of the 1960s, "socialism" was favored over "capitalism" by a margin of two to one.

The authors face similar conceptual difficulties in their chapter on partisanship in Venezuela. The concluding chapter concedes that their "evidence projects a rather complex and at times contradictory profile of the Venezuelan electorate" (p. 185). The authors might have been able to probe beyond these surface contradictions had they presented a more thorough and less tentative analysis of Venezuela's political party system. In one part of the book, they seem to accept the "clientelist nature of the major political parties" (p. 54). In their chapters on the ideological connection and partisanship, however, they imply a more programmatic and even ideological basis for the party system. One should not be too hard on either the authors or their Venezuelan sample. The contemporary Venezuelan democratic experiment commenced in 1958 and was a mere 15 years old in 1973. Party organizations in Venezuela are still quite unsettled. In addition, the Venezuelans surveyed for the purposes of this book were profoundly affected by the September 1973 Chilean coup and may have been in no mood to be totally consistent in their ideology and party leanings.

The Venezuelan political situation in 1979 is considerably different from that portrayed in this book. Democratic Action, the "populist" political party which had apparently regained its electoral hegemony in 1973, lost the presidency in the 1978 elections. In the June 1979 municipal elections, its decline continued as the party won only 30 percent of the vote. On the other hand, an invigorated and at least temporarily united leftist front doubled its vote in the June 1979 elections. The left, which was relatively insignificant in the 1973 portrait of the electorate, gained 20 percent of the vote in June 1979, and may be on its way to becoming a balance-of-power, third force in Venezuela's ongoing democratic experiment.

The findings of this book had a predictive ability regarding the electoral decline of Democratic Action in 1978 and 1979. The loyal, partisan active Venezuelan poor wanted more from their democracy than a piece of the patronage action. While rejecting a violent transformation of Venezuelan society (the Cuban case) and quite concerned about the risks of the politics of class confrontation (the Chilean case), the Venezuelan electorate in 1973, according to Baloyra and Martz, sought an active government leadership and was willing to make reasonable sacrifices for the sake of development and democracy. In 1973, it overwhelmingly restored Democratic Action to power in the hope that the grand old "populist" "party of the people" would carry through its historical mission.

Prospective readers who lack prior acquaintance with Venezuelan affairs might find the book difficult. For a specialist in Venezuela, this book is a delight.

DAVID EUGENE BLANK

University of Louisville

The Old Social Classes and the Revolutionary Movements in Iraq: A Study of Iraq's Old Landed and Commercial Classes and of its Communists, Ba'thists, and Free Officers. By Hanna Batatu. (Princeton, N.J.: Princeton University Press, 1979. Pp. xxiv + 1283. $75.00.)

Iraq is one of the most understudied countries in the Third World. Iraqi political patterns have long been a mystery to outsiders who have been forced to interpret events in that country largely through uninformed and distorted reports sporadically presented by the mass media. The rare scholarly writings about the Iraqi political system have tended to be traditional descriptive expositions or more modern analyses focusing on a few key political personalities and elite members. This famine of understanding is partly explained by the nature of the society itself. Observing Iraqi political processes can be likened to trying to view a constantly moving mosaic through a shaking kalaidoscope. In Iraq, ethnic, religions, class, personal, regional, and ideological coalitions are in a constant state of both formation and division. Few political scientists have been willing to tackle the complex task of explaining such a system. Indeed, very few have the linguistic and research tools to do so.

Against a backdrop of relative ignorance, the appearance of a 1283-page volume devoted entirely to the analysis of Iraqi political patterns is a major intellectual happening. Hanna Batatu's study is in fact three books. The first is essentially a study of the Iraqi upper class during the monarchical period (1921–1958). The second book is an analysis of the origins and development of the communist movement in Iraq. The final book discusses the interaction between the competing ideological groups focusing especially upon the Free Officers' movement. This concluding section of the volume brings the entire analysis surprisingly up-to-date. Batatu's overall analysis rests on an enormous storehouse of data gathered over the years from sources as rich and varied as the secret records of the Iraqi Directorate General of Internal Security along with personal interviews with many of the leading actors on the Iraqi political stage.

Batatu uses a class approach to the study of politics. The conceptual and theoretical underpinnings of this exercise are somewhat shaky and at times inconsistently applied. However, this is not a serious disadvantage, since the rather loose nature of the approach enables him to also focus in detail upon other critical social units such as personalities, families, and ethnic groups. In the end, the theoretical orientation is a decided emphasis upon social structure. Such an approach carries two major advantages which in the end enable this enormous volume to transcend its potential existence as a descriptive political dictionary or handbook. First, the focus is on the essence of politics and is not misplaced upon such externalities as voting behavior, formal-legal frameworks, and political attitudes described in a social vacuum. Batatu seldom strays far from discussion of the struggle for power and authority as manifested in his case study. Secondly, by constantly drawing relationships among individuals, groups, and classes, he is engaged in analysis and explanation rather than straight description and enumeration. In fact, the book is filled with hypotheses and explanatory theoretical statements at the middle level.

Among the major hypotheses are the following: Social transformation and political change in Iraq have been influenced most deeply by changes in the international economic market and the shifts in world industrial patterns. The extreme concentration of wealth at the very apex of the Iraqi social system during the monarchical period contributed to the violent and radical nature of opposition politics that mark the Iraqi political process to this very day. Iraq has long experienced a trend which has gradually disassociated the possession of property from political power. In the last two decades, the government has expanded deeply into the lives of the people. Simultaneously, there has been a dramatic proportional expansion in the size of the Iraqi middle classes. Despite this, the Iraqi government has yet to successfully mold the numerous disparate groups and classes into an integrated and working whole. In Batatu's own words, the success of any contemporary regime rests on "the necessity of binding the peasants to the townsmen and the Shīʿis to the Sunnis; and creating mutually advantageous relations between the Kurds and the Arabs; and, at the same time, raising qualitatively the standard of living of the mass of Iraqis. . ." (p. 1133).

The Old Social Classes is not an easy book to read. It will never stand accused of tightly-knit argumentation or compact, rigorous analysis. It is extremely repetitious as the same facts are marshaled constantly to support a slightly different generalization within a slightly different intellectual context. The somewhat rambling nature of the study is established already on p. 6 where the author manages to churn out a 111-word sentence. But Batatu is not unaware of this general problem, as he frankly states early in his preface: "Perhaps the exposition lapses here and there into minutiae or verges on a scholarly overkill" (p. xxi). And he then asks for the understanding of "the patient reader." This is a book worth being patient about.

JAMES A. BILL

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Biographical Dictionary of Modern British Radicals: Vol. 1, 1770–1830. Edited by Joseph O. Baylen and Norbert J. Grossman. (Atlantic Highlands, N.J.: Humanities Press, 1979. Pp. 565. $91.00.)

This reference work contains over 200 biographies of "radicals" written by 89 contributors drawn mainly from U.S. and British university faculties. The editors contributed also a brief introductory essay on the origins and development of radicalism during the period. They define radicalism broadly as "a state of mind, a sense of moral outrage against privilege, waste and abuse of power . . . sympathetic to change, impatient of argument based on custom and tradition" (p. 5). Thus, they include not only Bentham, Owen, and Paine, as would be expected, but also Blake, Shelley, and Southey. Blake, in fact, gets more space than either Cobbett or Ricardo. Surprisingly, however, Wilberforce is omitted. While the entries do not follow a standardized format, each typically includes an account of the main events in the individual's life, a summary of his (only five of those included are women) views, and an assessment of his significance or contribution. Also listed are major writings by and about him. For those beginning work on this period's politics, this volume doubtless will be an informative source, especially since the editors have endeavored in this volume to include people who typically do not appear in other similar standard biographical dictionaries. For those already conversant with the period, the bibliographic information may well be the volume's most useful feature.

JORGEN S. RASMUSSEN

Iowa State University

Constitutionalism in Asia: Asian Views of the American Influence. Edited by Lawrence Ward Beer. (Berkeley: University of California Press, 1979. Pp. x + 210. $18.75.)

A bicentennial program conducted in 1976 by the Committee on Asian Law, a committee of the Association for Asian Studies, brought to this country a number of eminent Asian jurists and scholars for a series of seminars and public meetings. Each has contributed to this symposium on American influence on constitutional development in his country. The result, if neither a skyrocket nor a sparkler (presumably appropriate imagery for a bicentennial occasion), does provide a couple of cheers for American constitutionalism.

What has been the extent and nature of the influence of the American constitution in environments quite unlike that in which it originated and developed? In the case of the Philippines, the influence has been direct and extensive, for understandable reasons. Supreme Court Justice Enrique Fernando stresses that such concepts as judicial review and a bill of rights were implanted early and inevitably found their way into the constitutions of 1935 and 1973. On the other hand, S. Jayakuma asserts there has been no direct American influence on constitutional formation and development in Singapore. Japan represents a special case, given the nature of American influence on the formation of the postwar constitution, but Nobushige Ukai confines himself to a consideration of the role of judicial review and the approach of the Supreme Court in individual rights cases. In the most extensive contribution, P. K. Tripathi gives a detailed account of the impact of American conceptions of judicial review and fundamental rights on the drafting of the Indian constitution with particular attention to the role of B. N. Rau, adviser to the Indian Constituent Assembly, who consulted widely with American jurists. Contributors from other countries (Bangladesh, the Republic of China, Indonesia, Malaysia) do not establish strong lines of influence though some general similarities are pointed out. Of these latter essays, the one by president of the Federal Court of Malaysia is particularly recommended for its welcome flashes of wit.

While the influence of fundamental rights is frequently cited, a number of contributors are critical of certain aspects of the American conception of them. They note that their own constitutions give greater stress to economic and social rights, that individual property rights need to be restricted in the interests of economic justice, that the welfare of society occasionally requires limitation on the freedom of expression. In short, many would strike a different balance between societal and individual concerns. Some are critical of the rigid nature of the American doctrine of separation of church and state. American principles of federalism, separation of powers, presidential government (with one or two exceptions), have had virtually no impact. In these areas, the colonial experience has been the more salient factor.

Constitutionalism is nowhere specifically defined, but from context, the contributors would seem to regard supremacy of constitutional law, judicial review, and fundamental rights as the critical elements. The approach, in other words, is legalistic. This is hardly surprising given their background, but the reader should not expect to find a discussion of social, political, or economic forces or a conception of a constitution as embodying what is written in the hearts of the people, as DeMaistre put it. The reader aware of political conditions in Asia will be struck at times by the gap between theory and practice—the treatment of the impact of martial law on fundamental rights in the Philippines, to cite just one example. In spite of its occasional wooden quality, the book should be of definite interest to all students of comparative law.

There are brief biographical sketches of the Asian contributors, a short bibliography, and the editor has provided a concise and useful summary in his introduction.

WILLARD H. ELSBREE

Ohio University

Algeria 1960: The Disenchantment of the World, The Sense of Honour, The Kabyle House or The World Reversed. By Pierre Bourdieu. Translated by Richard Nice. (New York: Cambridge University Press, 1979. Pp. viii + 158. $19.95.)

To write of the disenchantment of the world is also to envision a world enchanted, with time unfolding as a "moving image of eternity" (p. 26). On the one hand the ethnographer's "structural analysis" captures a traditional order, that of Kabyle peasants as they may have once been, which "seeks to abolish chronological succession in the sense of continuous discontinuity, by reducing the chronological order (in the cycle of life as in the cycle of farming) to the (mytho) logical order" (p. 29). On the other hand, his synchronic snapshot of the economic dispositions of a cross section of Algerians, circa 1960, is intended to reveal the diachronic stages of "the process of 'rationalization' " (p. 64), whereby workers achieve consciousness of their revolutionary role. Whatever Bourdieu's original political intentions, this collection of essays will interest the sociologist of a certain science as much as the student of contemporary Algeria.

The book is a rehash, but worth the rereading, of research conducted two decades ago and long since published, some of it already in English. The principal essay, "The Disenchantment of the World," otherwise unavailable in English, is a sanitized version of *Travail et Travailleurs en Algérie* (Paris: Mouton, 1963), which also recently reappeared in French as *Algérie 60* (Paris: Editions de Minuit, 1978). It is sanitized in that there are no longer any disparaging references to the Algerian "révolution à demi" (cf. *Travail*, p. 389) of a petite bourgeoisie that could not quite make sense of history. Bourdieu still tries to explain, however, why "the proletarianized peasantry and the urban sub-proletariat do not constitute a revolutionary force in the true sense" (p. 62). Their precarious economic situation does not allow them to make the rational calculations characteristic of a capitalist economy, much less achieve the class-consciousness expected of Marxist revolutionaries. Why many of them nonetheless participated in the Algerian Revolution of 1954–1962 is not explained in this essay. Within Bourdieu's logic, Algerians responding to his survey in the late 1950s who blamed their economic difficulties on the Europeans or the French were just being irrational: "Absolute alienation annihilates even awareness of being alienated" (p. 61). However, Bourdieu's concrete examples of rational calculation, or lack thereof, do not always seem convincing. The respondent on p. 66, for instance, who has debts of almost 5,000 (old) francs and earns 10,000 francs per month on average, however precariously, hardly seems less capable of calculating and forecasting than the average American consumer. Conversely, the better-off individual on p. 54 who rationally circumscribes his children's future to engineering or technical careers, rather than medicine, may be limiting them only in light of Bourdieu's possible bias in favor of doctors and lawyers (cf. p. 69). "A certain type of sociology," to reuse the label he conferred upon Daniel Lerner, should be of interest to the colonial historian.

It is also relevant to contemporary Algeria. That traditional Kabyle peasant society, for instance, "has no place for the experience of unemployment" (p. 41), may help to explain why the Algerian government, despite its capital-intensive development strategy, has been less forthcoming than its neighbors in admitting an employment problem. The fact, too, that traditional peasants are less equipped than agricultural workers for collective farming (p. 14) may help to explain the failure of the new *villages socialistes* to take root. Urban housing remains a major problem; the tenement housing mistranslated in the text as "estates" remains the obstacle to entering modern life which Bourdieu so sensitively described. *Piston* (or pull) also remains an omnipresent factor in daily dealings with a Napoleonic administration, and Bourdieu is probably still correct in suggesting that it works best for clerical workers and supervisory staff (p. 36). His description of Algeria as transitional, not yet truly capitalist and capable of generating a proletarian revolution, is also probably truer today, given the demographic problem, than it was two decades ago.

Fortunately, little emphasis is placed in the present text upon the survey on which it is ostensibly based, but the synoptic tables on pp. 38 and 76 might have admitted the number of respondents to have been only 150 or so, rather than simply referring the reader to *Travail* for such petty methodological details.

CLEMENT HENRY MOORE

University of Michigan

The Super-Bureaucrats: Structure and Behaviour in Central Agencies. By Colin Campbell and George J. Szablowski. (New York: New York University Press, 1979. Pp. x + 286. $9.95, paper.)

Central coordinating agencies charged with the mission of bridging analytical and political elements of policy have come into being in all of the industrial democracies. *The Super-Bureaucrats* focuses on the performance of these influential agencies in Canada. Colin Campbell and George Szablowski interviewed elite officials in five federal super agencies in Canada: the Prime Minister's Office (PMO), the Privy Council Office (PCO), the Federal-Provincial Relations Office (FPRO), the Treasury Board Secretariat (TBS), and a powerful coordinative line agency, the Finance Ministry. Their data are then used to analyze the composition, recruitment paths, perceptions, beliefs, and self-reported behavior of high officials in these agencies.

One troubling question that arises from the growth of these nontraditional centers of decisional and agenda-setting power is to what extent the officials who populate these centers constitute a highly integrated elite which brings to bear common understandings, perspectives, and criteria in dealing with public policy problems? Democratic control of bureaucracy is, of course, an issue of long standing. But it is particularly pertinent in the case of the coordinating agencies, since they rarely operate under statutory guidelines. Through much of their analysis, therefore, the authors implicitly ask three questions: (1) are elite officials in the super agencies different from their peers in the line agencies? (2) do elite officials in the super agencies differ from one another across agency lines? and (3) to what extent are Canadian elite officials different from their European counterparts? While their own samples permit the authors to answer only question 2 directly, whenever possible they deal with questions 1 and 3 by making judicious use of published research findings on members of the Canadian, United States, and European bureaucratic elite. From background and career data, the authors note that officials in the central agencies are more likely than those in the line agencies to be younger, to be ethnically and regionally diverse, to be "irregulars" from outside the traditional civil service, and to be professionally educated. Central agency officials also are less likely to be client-centered, less likely to think of politics and administration as separable activities, and more inclined to exhibit loyalty to the government than to a department. When contrasted with their European counterparts, however, even top Canadian line civil servants show less evidence of a guild-like public service career path and, thus, less evidence also of social homogeneity and other earmarks of an integrated elite. In this respect, the Canadian civil service elite more closely resembles that of the United States than of European systems.

Although, understandably, high officials in the super agencies are in an advantaged position to manipulate agendas and decision levers, the fear that they do so on behalf of a cohesive outlook is misplaced. For the problem of accountability that is raised is not this obvious one; rather, it is that decisional discretion in the absence of clear guidelines and of operational capacities to perform the missions with which these agencies are charged fosters bureaucratic freelancing. Ironically, therefore, one deficiency that the authors perceive in the operations of the central agencies is that there is too little commonality of purpose and too little system-wide perspective among central agency officials. At the same time, great influence is wielded by the super-bureaucrats though it is mostly concealed from public visibility and institutionalized scrutiny.

Campbell and Szablowski conclude, consequently, with specific prescriptions for enhancing the accountability of central agencies, though they define accountability in diverse and seemingly incompatible ways. While the specific reforms they offer are of primary importance to students of Canadian government, their broad thrust is of wider interest. Traditional notions of ministerial responsibility are viewed by the authors as frequently ineffective and, in any event, not highly relevant to the operations of the central agencies. Strengthening central agency accountability, then, is thought of in terms of (1) promoting selection and socialization policies that facilitate a sensitivity to the broad public interest, and (2) increasing and regularizing parliamentary intervention.

The first prescription for enhancing accountability, i.e., more integration, may have many virtues, but promoting accountability seems unlikely to be one of them. As for the second prescription, increased parliamentary activism in fact signifies fundamental institutional change. It is, at once, likely to undermine both what remains of the idea of ministerial responsibility and the prospects for a more coherent blend of policy analysis and political sensitivity emerging from the central agencies. The authors seem here to have taken a fancy to the visibility of administrative decision making prevalent in the United States and to the oversight role of the Congress. They might be advised to leave well enough alone. It is certainly hard to imagine how parliamentary showcasing of central agency officials will lead to better policy analysis. It is easier to imagine how it will lead to greater political sensitivity, but then the problem becomes one of assessing whose political needs central agency officials should be sensitive to. This particular issue, as some now unfashionable theories of administration claimed, is not one that administrators ought to be faced with.

Despite the analytic traps inherent in wrestling with problems of administrative accountability which sometimes ensnarl the authors, *The Super-Bureaucrats* is a significant addition to the empirical literature dealing with comparative elites and comparative bureaucracy.

BERT A. ROCKMAN

University of Pittsburgh

Indira Gandhi in the Crucible of Leadership: A Political Biography. By Mary C. Carras. (Boston: Beacon Press, 1979. Pp. xvi + 289. $13.95.)

Indira Gandhi's tenure as prime minister of the world's second-largest country ended with 21 months of repressive Emergency rule (1975–1977) and electoral defeat. In recent months, Mrs. Gandhi's political fortunes have appeared to improve. While it remains to be seen whether she, like Richard Nixon, can arise phoenix-like from her own political ashes, her continuing prominence in Indian public life is sufficient to commend this book for its timeliness and relevance.

The designation "political biography" is not quite appropriate, however. The primary purpose of the book appears to be to explain, even exonerate, Gandhi's actions in declaring the June 1975 emergency. The first three chapters do attempt a psycho-biographical assessment of her personality, but they do so in order to show that (1) she was not power-hungry, but rather achievement-motivated; and (2) she was not authoritarian, but rather had "a democratic temperament and personal style," with, however, "a deep ambivalence regarding the proper exercise of power" (p. 100). Mary Carras draws fairly heavily upon standard Gandhi biographies (e.g., Bhatia, Masani, Hutheesing) and social psychology (e.g., Harvey, Maslow, Davies), but seemingly ignores two previous psycho-political assessments of Mrs. Gandhi (by Nayantara Sehgal—Mrs. Gandhi's cousin—and by Henry Hart) which are less sympathetic than Carras' treatment.

Having eliminated "authoritarian personality" explanations from consideration, in the following four chapters Carras shows that the "causes" of the Emergency might plausibly be found in external events, such as the irresponsibility of the opposition parties and the progressive breakdown of political order in India in the late sixties and early seventies. Throughout, Mrs. Gandhi is portrayed as a well-meaning socialist whose only *faults* are her willingness to compromise and her reluctance to wield power ruthlessly! These chapters, while not biography, provide rich detail on the political history of the Indira decade. However, they somehow fail to mention the increasing centralization of power during that period in the hands of the prime minister (at the expense of the state chief ministers) or the adulatory "great leader" slogans which began to appear in Delhi and elsewhere at least as early as 1973. J. P. Narayan's mass protest movement in Bihar is treated as the major precipitating cause of the Emergency: "The possibility of chaos and violence, which could threaten both the Indian political institutions and economic stability, loomed large. The stakes were high. The prime minister had to respond quickly and firmly to the challenge" (p. 203). The other major challenge faced by Mrs. Gandhi at that time—a high court judgment finding her guilty of election malpractice in 1971—is dismissed by Carras with the observation that the decision was later (during the Emergency) overturned by the Indian Supreme Court (p. 205).

The final two chapters appear more in the nature of appendices, but serve well to complement the preceding argument. One is an analysis and assessment of the Shah Commission's 1977–78 enquiries into Mrs. Gandhi's emergency declaration. The second is an interview of Gandhi by Carras.

Despite its one-sided pro-Indira treatment, this book is worth reading. It is well written and its argument is the fullest, most sympathetic, and most persuasive justification available of Mrs. Gandhi's actions in 1975. Richard Nixon would no doubt like to have such a political biographer.

WILLIAM L. RICHTER

Kansas State University

Ideology and Politics: The Socialist Party of France. By George A. Codding, Jr. and William Safran. (Boulder, Colo.: Westview Press, 1979. Pp. xvi + 280. $18.50.)

The French Socialists have attracted a good deal of attention since their reorganization, first in 1969 and then again in 1971. Although there have been numerous articles on the ideology, composition and policies of the Socialists in both the scholarly and popular press, *Ideology and Politics: The Socialist Party of France* is the first general history of French democratic socialists to appear in English in a decade. This fact alone makes the book noteworthy for students of West European politics.

Ideology and Politics is a history of the French Socialist party during the twentieth century. A "descriptive analysis," as the authors call it (p. 243), the book is organized largely along chronological lines. More than three-fourths of the book treats the evolution of the French Section of the Workers' International, or SFIO (*Section française de l'Internationale ouvrière*), the official name of the party from 1920 to 1969. The last two chapters treat the new Socialist party (PS), and draw conclusions about the significance of the party's evolution. The book is divided into three parts: the background of the SFIO, including its ideology and organization; the role of the SFIO in the Fourth Republic and the beginning of the Fifth Republic; and the "revitalization" of the Socialists in the 1960s and 1970s.

There is little new or surprising information in the study. The authors state that their aim is to provide "a case study of this political party from its inception to the present" (p. 2). Insofar as the "case study" of a party implies its history, there is no question that the authors have realized their objective. But insofar as the authors had more ambitious goals, the study falls short. In their introduction, Codding and Safran suggest that the study should "furnish the reader with an insight into the major developments in French political life over the past several decades," and that it might supply "clues to the tendency of socialist parties all over Europe to merge into larger units or to water down their ideology. . ." (p. 2). These are reasonable goals. If a case study is to provide insights into the larger issues of politics, it is reasonable to expect it to explain some characteristics of the political system within which the party functions and/or to add to our knowledge of other parties which resemble the party under study.

Codding and Safran partially achieve both objectives, but only indirectly. For example, the problem of the role of ideology in social democratic parties is suggested throughout the book, particularly as we see the French Socialists wrestling with the question of participating in the non-socialist governments of the Third and Fourth Republics. But because there is no systematic examination of the role of *Marxist* ideology in *social democratic* parties, it is occasionally difficult to understand the seemingly endless efforts of the French Socialists to define their niche between the Communists and the center parties in French politics. Without such a systematic analysis, the concluding chapter, entitled "Comparisons and Perspectives," raises some provocative questions, but on balance is more confusing than enlightening. The authors recall the appeal of pragmatism and opportunism for the Socialists, a point which is obvious in the light of the historical events described in the book. But the authors also accept the notion that the Socialists' ideological orientations "point to certain options and exclude others" (p. 252). This observation is then followed by the unsubstantiated statement that "there is no question that left-wing parties tend on the whole to be more humane and more interested in efficiency than right-wing parties" (p. 252). Coming after a discussion of the SFIO's decline, a decline accelerated by the party's support for the war in Algeria during the 1950s (a war conceded to be neither humane nor efficient), the reader is still left with some confusion as to the relative importance of opportunism and ideology for the French Socialists. A more theoretical orientation would have facilitated linking this case study with the kinds of questions which students of communist parties, for example, typically raise: the problem of maintaining party legitimacy in a bourgeois democratic regime; the "end of ideology" debate; the impact of institutional changes on party fortunes; or the historical deradicalization of Marxist parties.

Some parts of the book are especially welcome, even in a general history. In explaining the SFIO's official ideology, a section on "Economic Theory" (pp. 42–47) includes material rarely seen in English-language studies of the Socialists. A chapter on organization concludes with a description of Guy Mollet, the Secretary-General of the SFIO from 1946 to 1969. Although it is not entirely clear how Mollet's "doctrinaire" positions (pp. 84, 155, 170) are reconciled with his "republican realism" (p. 154) or his outright opportunism (pp. 154, 180), Codding's and Safran's discussion should at least spark a fresh look at a complex man who played a complex role in French politics for more than two decades.

Although the detailed recounting of the SFIO's political maneuvering verges on the tedious in places, it is part of the price of a comprehensive history. Those not drawn to the "ins" and "outs" of French Socialist politics in the Fourth Republic might prefer to concentrate on the first and third sections of the book. All readers should find the selected bibliography helpful.

SUE ELLEN M. CHARLTON

Colorado State University

Class Conflict and the Industrial Relations Crisis: Compromise and Corporatism in the Policies of the British State. By Colin Crouch. (Atlantic Highlands, N.J.: Humanities Press, 1977. Pp. xvii + 302. $15.00.)

Incomes policies in Western societies, and especially within the United Kingdom, have been the subject of intense study and debate. The majority of these studies have concentrated on the politics of formulating these policies, or on the economic effects of the policies on employees and national aggregate such as inflation. Crouch's approach is distinctly sociological, and the most important portion of the book is the attempt to develop a set of "dilemmas" covering the major aspects of the relationship between industrial relations and class relations.

There are four such "dilemmas." The first is the "employment relationship," or the relationship between employers and employees. This involves the demand for order at the workplace, the demand for involvement of workers in the activities of the enterprise, as well as the degree of pluralism accepted in the workplace and the degree to which the position of the two major actors in the employment relationship are legally defined. The second major dilemma is that of the role of the state in employment relationships. The state may be coercive or non-coercive, and may be active or passive in its dealings both with employment practices and the society as a whole. The third dimension or dilemma to be considered is the organization of subordinates in the employment relationship. Hence we must consider the individualistic persuasions of the work force (or the lack of them), the autonomy of employees' organizations, and the legal status of workers' organizations. Finally, we must consider the structuring of inequality within society, and the extent to which existing inequalities are justified on ideological grounds, the result of public policies or market forces, systematic or ad hoc in this impact, and the hostage to economic growth for any possible change.

Taken together, the above four dilemmas tend to produce, in Crouch's mind, six major forms of industrial relations systems in industrialized societies. As formulated, these are ideal types which may not exist in pure forms in any society but which serve as useful points of comparison for real-world systems. The first is laissez-faire, or "Market Individualism," a system requiring little further description. Two of the ideal types developed are labelled "Liberal Collectivism," implying a system differing from market individualism primarily in the greater organization of employees. There are two variants of Liberal Collectivism: one in which the employee's organizations are clearly dominated by the employer, and one in which relative equality exist. The final three systems of industrial relations are varous forms of Corporatism, labelled "Statist," "Voluntarist" and "Private" corporatism. The first two are roughly equivalent to Schmitter's State and Societal Corporatism, while the last assumes domination by employers, albeit in a very different manner than in the laissez-faire system.

Crouch describes the general framework of industrial relations as "the Compromise." This is actually a particular British version of liberal collectivism, reflecting features such as the absence of major labor legislation and the tendency of labor leaders to be coopted into the national elite.

The majority of the book is a discussion of various incomes policies which have existed in Britain since the late 1950s. These policies are described as to their intentions, concepts of equity, and their relationship to "the Compromise." In general, these descriptions demonstrate a shift from the liberal collectivist to the corporatist forms of industrial relations.

This book represents a very thorough job of research, and a more complete theoretical perspective on incomes policies than is usually found. However, at times the theoretical framework obscures some basic questions and interferes with an understanding of the *political* dynamics of incomes policies. The framework developed provides extremely good descriptions of static policies, but is of little use in explaining changes from one policy to another. In particular, we gain very little understanding of the reasons for the growth of corporatist solutions to state-society relationships in Britain, and by inference in the rest of Western Europe. This is a valuable conceptual effort at classification, but would have been a much more valuable book if it had dealt with the fundamental political problems involved in changing systems of industrial relations.

B. GUY PETERS

Tulane University

China after the Cultural Revolution: Politics between Two Party Congresses. By Jürgen Domes. Translated by Annette Berg and David Goodman. (Berkeley: University of California Press, 1977. Pp. viii + 283. $15.00.)

This book by the prolific West German China scholar Jürgen Domes is among the few that have hitherto appeared which are specifically addressed to the period following the Cultural Revolution. And unlike the studies by A. Doak Barnett (1974) and J. P. Jain (1975), it is not specifically focused around the issue of succession, but seeks rather to encompass the broader social processes of post-revolutionary readjustment, including even a section by contributing author Marie-Luise Naeth on recent foreign policy developments. For this reason alone, it is to be welcomed. Also praiseworthy is the methodological virtuosity of the author, who combines intensive "Kremlinological" textual exigesis with the quantitative analysis of elite data. On the whole, the book is well documented.

The book is an English translation (with a two-page epilogue) of a German edition that went to press in 1974, and some of the chapters are minor revisions of journal articles first published in 1969 (Ch. 2), 1971 (Ch. 1) or 1973 (Ch. 3). Hence the meeting of the 4th National People's Congress in January 1975, the deaths of Zhou Enlai and Mao Zedong and subsequent struggles, are all necessarily omitted from its purview. Domes is essentially concerned with the rise and fall of Lin Biao and with the events that attended this: the People's Liberation Army's seizure of civilian power in the wake of the anarchy of the Cultural Revolution, Lin Biao's coronation as Mao's new crown prince at the 9th Party Congress in 1969, Lin's falling-out with Mao, his alleged conspiracy and its fatal undoing, the subsequent cautious removal of his supporters from their key positions in the apparatus, and so forth.

The study is on the whole descriptive, not an attempt to contribute to a more general theory of post-revolutionary readjustment. Domes does make an attempt to endow his work with more general significance by relating it to Max Weber's discussion of the routinization of charisma in his conclusion, but this conclusion is unfortunately not systematically related to the individual chapters. Any attempt to relate his account to Chinese attempts to understand or describe their own political system he dismisses as ingenuous, adopting a sharp distinction between ideology and science: As he states in his introduction, ideology "ought to be seen as the manipulative instrument for the justification of power politics" (p. 3). To understand power politics, Domes proceeds to accumulate a great mass of details that are thought to signal in various ways the ebb and flow of power. Primarily in focus are of course the patterns of personnel appointments to strategic positions in the government apparatus, which redound to the advantage of the various informal groups that are thought to be the actual focus of political loyalty. Policies may, however, also be used to build or expand constituencies, and the delicate nuances of ceremonial rhetoric are examined for what they reveal about the policy direction in which particular leaders may be leaning. Although policies and sometimes even popular response to policies are thus sometimes brought into play, the independent variable is invariably power: *kto kvo.*

These techniques are quite familiar to China watchers, and may be appropriate to the Lin Biao episode, which seems to have exemplified power politics par excellence. In his interpretation of the incident Domes essentially agrees with the previous accounts by Philip Bridgham (1973) and Ying-mao Kau (1975). But he differs with these interpretations in one important respect: he conceives Lin's ambitions to have included not merely power-political objectives, but a radical redistribution program in China's rural sector. Lin is said to have introduced a systematic program in the spring of 1971 which entailed the roll-back of private plots, the allocation of work points on the basis of political rather than productive criteria, and the elevation of the level of ownership and accountability from the team to the brigade or commune. Domes' evidence to indicate Lin's support for such a program is quite thin, however, and it is not psychologically obvious why an ambitious power-seeker should adopt a policy that militated against the interests of the regional military leaders who constituted his principal base of support. But Lin was a leftist and therefore committed to such a program on non-rational grounds, Domes implies. But if this is true, it begs the question of Lin's split with Mao Zedong, and with the civilian leftists now known as the "Gang of Four." To explain this, Domes reverts from his brief flirtation with ideological explanations to more familiar power-political considerations: Mao's suspicions were aroused by Lin's attempt in September 1970 to nominate Mao to the chairmanship of the State, a plot whereby Lin could become chairman upon Mao's expected refusal. If this is so, Domes' inference that Mao did not split with Lin until the spring of 1971 seems quite unaccountable—and contrary to Mao's own testimony in his speeches to local military leaders in August to September 1971 that he had undertaken a series of preemptive moves to corner and eliminate Lin following the Lushan plenum. Perhaps Domes was misled here by his tendency to concentrate on provincial radio broadcasts and to infer national policy initiatives from them, thus attributing national importance to a few local outbreaks of radical agitation that never received any form of public support from the central authorities. The right of the peasants to keep their private plots of land, to retain the three-level ownership system with the team as the basic unit of accountability, and to participate in rural free markets, was defended in the constitution of the 9th Party Congress and again in the draft state constitution presented at the 2nd Plenum in September 1970.

Notwithstanding its aspiration to scientific objectivity, the book is informed by a pervasive sense of cynicism toward the claims of ideology that ultimately lends to its own hard-bitten bias. For example, PLA soldiers are habitually referred to as "storm-troops" (evoking images of jack-booted Nazi *Sturmabteilungen*), and Domes seems to provide invariably sinister explanations for whatever appears inexplicable (for example, on p. 97 he infers that General Tan Furen was "probably murdered" because "the Chinese media mentioned his death without mentioning, as usually happens, that he died from a serious illness"). The "individualized and monolithic concept" of Chinese leadership that was previously embraced by "the majority of China scholars" (p. 3) is discarded in favor of a conflict model that tends to impute the factionalism and intrigue that became rampant during the Cultural Revolution to the routine Chinese policy-making process. This leads Domes to understand routine politics as crisis politics, with a corresponding tendency to magnify all conceivable manifestations of conflict or disagreement, to overstate the degree of policy oscillation over time (e.g., see pp. 162 ff., where he christens certain agricultural politics that had never really been rescinded as a "new course"), and to exaggerate the tenacity and permanence of factional alignments (which may well be much more tentative and expediential in normal bureaucratic decision making than in public clashes between conflict groups).

LOWELL DITTMER

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Cuba: Order and Revolution. By Jorge I. Domínguez. (Cambridge, Mass.: Harvard University Press, 1978. Pp. xix + 683. $25.00.)

This is a massive and serious book. The result of a dozen years of work, albeit intermittent, it is impressive by virtue of its sheer bulk, its ambitiousness, and the ingenuity, quantity, and variety of the data presented. Ninety tables are sprinkled across its more than 500 pages of regular text. Footnotes occupy almost 90 pages more of fine print, and the bibliography goes on for yet another 36 pages. Domínguez's work is weighty, both literally and figuratively.

The book is organized into three parts: The first, entitled "Prerevolutionary Cuba," is a useful 120-page introduction to Cuba before Castro. Specialists in the period might find much to quarrel with in Domínguez's presentation, but he certainly covers the main events, actors, and the dynamics of the period from 1902 to 1958. The second part, entitled "Revolutionary Cuba: Governing through Centralization," is in many ways the heart of the book. Largely abandoning the chronological structure of part 1, the author instead opts for a topical—one might even say institutional—approach to the revolution. Chapters 7, 8, and 9 (the final three of the five in this section) suggest this topical/institutional focus: "Mass Political Participation," "The Communist Party," and "The Civic Soldier."

The final part of the book is called "Political Processes and Change." Here the coverage is somewhat spottier than before, with one chapter on the making of public policy, one on agrarian conflict and peasant politics, and one on political culture. This final chapter on political culture attempts additional topical coverage through subsections on students, women, and mention of various other policy domains such as health. But still one is left with a sense (which Domínguez himself surely shares) of important questions of process and change left untouched.

A book as long and detailed as this cannot be summarized briefly. It is fair to say, however, that the main themes to which Domínguez constantly returns are relatively few and quite straightforward, while—it should be added—they are also very controversial. One is a cautionary theme, stating in effect that despite the tremendous convolutions undergone by Cuban society, life at the micro-level has changed less than one might expect. Thus, the countryside is still much poorer than the cities; blacks are still relatively under-represented in high-status positions; much of the old macho culture still prevails; improvements in health, education, and welfare are not as dramatic as Cuban revolutionaries and their friends would have us believe.

A second theme focuses on the organization of power, emphasizing the hierarchical, centralized, mobilizational but not fully participatory style of politics practiced by the revolutionary elite. A third theme stresses the much-transformed foreign relations of the Cuban Revolution, and—by implication—the crucial role played by the Soviets in the last decade of Cuban history. Again and again these themes are buttressed with examples, statistics, small discourses on one or another aspect of revolutionary politics and society. A book of equal length could be written extending, criticizing, and rebutting this multitude of arguments.

Precisely because Domínguez is so clever in finding bits and pieces of data to support his interpretations of the revolutionary process, however, and because he is so encyclopedic in his sources, we must step back from the work as a whole and ask a very basic question: what kind of book is this? At the risk of some oversimplification, I would say that it is standard American political science of the 1960s and early 1970s: subspecies comparative/developmental. That is, it is a loosely articulated set of concepts and ideas about how politics operates in a so-called less-developed society, "tested" and elaborated by data on micro aspects of system functioning. Unfortunately, this never-very-powerful intellectual tradition is here applied to the analysis of phenomena which yield even more grudgingly than usual to the theory and methods used. More specifically, what does this mean?

It means that Domínguez approaches the Cuban Revolution in a very functionalist, middle-range, and ultimately empiricist way. He has certain hypotheses about important actors, how they operate, the main linkages between the political system and society, and the important domains of social and political change. He then sets out to find data that illuminate these actors, linkages, and domains. But this approach applied to a transformation as profound as the Cuban Revolution has serious—one might say fatal—limitations.

In the first place, the explicit and at times implicit model of social change and state-society relationships that Domínguez uses cannot penetrate to the core of revolutionary dynamics. One does not have to accept Marxism as an analytical framework to appreciate that Cuba has undergone a revolutionary change in the classical Marxist sense. That is, there has been a structural transformation which fundamentally altered the mode of production, and thus redefined the meaning of class and class relations. Any thorough and holistic analysis of the Cuban revolution *must* probe much more deeply into these fundamental transformations of the Cuban economy and class system than Domínguez's framework encourages him to do or allows. Since these transformations energize and condition almost everything else that has happened, failing to place them at the theoretical core of the analysis leads to endless misunderstandings of lesser and derivative phenomena.

In the second place—and this is related to the preceding comment—Domínguez's approach does not lead him to locate the Cuban Revolution as fully or deeply as it should be in the international system, especially in the context of U.S. imperialism during the 1960s and 1970s. In a complex but very basic way, U.S. hostility to the Cuban experiment shaped the institutions and processes that Domínguez deals with. Cuban socialism and Cuban politics were, and continue to be, a response not only to a penetrated and dependent (and maldeveloped) Cuban capitalism, but also to the world capitalist system, its values, politics, and continuing attempts to dominate. Superficially Domínguez realizes this, but the connections are deeper and more subtle than his book would lead us to believe.

In the third place, the rampant empiricism of the author's approach can only lead to eventual frustration, misinterpretations, and misunderstandings of various phenomena. Domínguez wants and usually finds or invents an indicator for everything: pictures in a Cuban magazine to suggest that blacks are under-represented among officers in the Cuban armed forces; pre- and post-revolutionary statistics on joint authorship of scientific articles to suggest that not too much has changed in patterns of scientific cooperation over the last quarter century, and so forth. But data of this sort are subject to multiple interpretations, and as we should now know from endless critiques of empiricism, the key methodological question is whether in real life the data have anything at all to do with the phenomenon allegedly under examination.

In sum, in the long run *Cuba: Order and Revolution* falls victim to its own theory and method. The book disappoints not because the author is not learned, not because he is not conscientious, not because he is not (occasionally) sympathetic to revolutionary achievemnts. Rather it is ultimately unsatisfying, thin stuff because the author's approach fails to lead him to many of the most important questions, and in fact, even dooms him to finding only partial answers to many of those secondary questions that he actually does ask.

On the last point, I extend my hand in sympathy. As one who attempted empirical work on the Cuban revolution many years ago, I know how difficult it is to collect relevant materials, how questionable the inferences from these materials must often be, how much the revolutionaries themselves have chosen to hide. I also know that Domínguez was not able to reenter Cuba (he left as a 15-year-old in 1960) until after this manuscript was completed. But it is also true that even research on the island would not have significantly altered the main arguments and conclusions of this massive work.

Thus despite the dust jacket claim that this is "the definitive study on twentieth century Cuba," we are still awaiting such a book. When and if it comes, however, it will not be written by someone who approaches revolutionary Cuba with the perspectives and tools used in this volume. The definitive study will, instead, be a political-economic analysis which takes Cuba's socialism more seriously, which understands that when the revolution destroyed capitalism and capitalist relations of production, it also destroyed any possibility of really understanding contemporary Cuba with theories and methods designed for other times and other places.

RICHARD R. FAGEN

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Germany in World Politics. Edited by Viola Herms Drath. (New York: Cyrco Press, 1979. Pp. xii + 282. $15.95, cloth; $9.95, paper.)

This is a good book with a misleading title. Only a third of the readings by 16 authors deal with East and West Germany's political and economic relations with the rest of the world. However, this book is especially valuable precisely because its scope was broadened to include important aspects of German society, culture, and political history.

It is not surprising that there is no overall argument in this diverse collection. Nevertheless, pictures of the two Germanies emerge which upset a few prevailing assumptions. Certainly this is true in the case of West Germany. East Germany (which is treated in only two essays) is portrayed as heavily ideological, but at the same time devoted in practice to "living peacefully side by side" with West Germany. East German political leaders (in contrast to their subjects) openly oppose the concept of a unified German state or nation. Despite the widespread international recognition the regime has received in the 1970s, its leaders continue to fear the influence of the West, especially of West Germany, and therefore can be expected to maintain a repressive policy toward their own people.

The cliché that West Germany is an "economic giant and a political dwarf" is outdated. Less restrained by the embarrassing German past and confronted with a diminution of French and British power and Washington's "self-absorption" during much of the past decade, the Federal Republic increasingly shoulders political responsibilities and exercises diplomatic leverage throughout the world. Of course, Washington's "Bonn connection," which replaced the United States' former special relationship with Britain, continues to be crucial for West Germany's military security. Such security has enabled Bonn to deal confidently with East European nations and the Soviet Union since 1969. What is new is that West Germany is willing to say no to the United States, or to demand explicit trade-offs for its concessions. Also new is Bonn's scarcely concealed impatience with the American decision-making process.

With a robust economy, a secure footing in the Western alliance, an ongoing process of rapprochement with its East European neighbors and good relations with both superpowers, West Germany approaches world political problems with a new self-assurance. Indeed, Helmut Schmidt's own image of confidence blends well with the image his country presents to the outside world. What remains unanswered, however, is the extent to which the Atlantic Alliance will be willing to change to accommodate a more active Federal Republic. Might this increasingly active role constitute a challenge to U.S. leadership within the alliance? Might it lead to a "bigemony" within the alliance which could revive old resentments or generate new fears among its European neighbors?

Such self-assurance does not necessarily carry over into all aspects of West German society or culture. The churches, which were the only institutions to survive the Third Reich practically unscathed, have contributed significantly to establishing a moderate and compromising political culture; however, questions remain concerning the extent to which they (especially the Catholic Church) should actively enter the political arena. The elite structure of West German schools remains despite important educational reforms. Also, West German schools have not been entirely successful in providing young people with an adequate appreciation of pluralist political democracy. Although women have gained full legal equality, they often continue to suffer from discrimination on the job and inequality at home.

The lack of self-assurance and optimism is particularly manifested in contemporary German literature, which is characterized by anxiety, malaise, self-doubt and social criticism. The majority of German writers today show impatience with traditional literary themes and styles, rejects fixed criteria, focus sharply and without humor on the individual's problems in society, and above all severely criticize West German society, which is often viewed as unjust and hostile to individual freedom and identity. Many German writers, especially young ones, are highly political and see themselves as initiators of resistance to the social order. Hugo Mueller and Bruno F. Steinbruckner argue in their essays that social criticism can have many different forms and objectives and that social criticism and political themes in general are by no means new in German literature since 1945. Nevertheless, the overall picture of West Germany presented in its contemporary literature is that of a sick society, and as Kurt Sontheimer argues in his recent works on intellectuals in the Federal Republic, such a picture inevitably affects a country's political culture.

This is a well-edited book in which each piece is tightly organized and ends with a prognosis. Political scientists will find the pieces by Catherine Kelleher, Peter Lutz, John Starrels and Ralf Dahrendorf especially valuable. As can be expected, the quality of the individual contributions varies, but the book's overall portrayal of contemporary Germany and its problems is accurate.

WAYNE C. THOMPSON

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The Future of North America: Canada, the United States, and Quebec Nationalism. Edited by Elliot J. Feldman and Neil Nevitte. (Cambridge, Mass.: Harvard University Center for International Affairs, and Montreal: Institute for Research on Public Policy, 1979. Pp. 378. $13.95, cloth; $6.95, paper.)

This book represents the proceedings of a seminar on Canada-United States relations held at Harvard University in 1977–78 and chaired by the co-editors of the volume. The seminar was an ambitious one, attempting to deal simultaneously with the twin themes of the tensions within Canadian Confederation brought about by the election of the Parti Quebecois in 1976 and the periodic tensions in Canada-U.S. relations related to issues such as trade, investment, and communications. Only three of the 13 contributed chapters (exlcuding the substantial introductory and concluding essays by the co-editors) deal with Canadian-American relations, however, so the book may more properly be thought of as a series of essays on contemporary Canadian federalism intended primarily (but not exclusively) for a U.S. audience. This balance is attributable partly to the high salience which issues concerning the future of Canadian federalism have had in Canada in recent years and the corresponding low salience of the key foreign policy issues on the current political scene.

The volume also reflects the unique and imaginative format of the conference from which the contributed essays were drawn. The substantive essays are by Canadian authors with considerable first-hand experience in their particular topical areas, either in government or academia. Each substantive essay is followed by a commentary, in most cases written by an American participant in the seminar who brings to bear on the topic expertise in the subject area—energy policy, communications, foreign policy, etc.—rather than broad background in Canadian affairs. This provides, in the view of the editors, a "comparative-American perspective" on the viewpoint advanced in the Canadian contribution. This format is intended to create a sense of interchange between the writer and the critic, and to involve the reader in the dialogue.

At its best, the approach gives the reader, particularly the Canadian reader, a sense of a quite different perspective and approach when a familiar problem or concern is seen through American eyes. But the method does not always succeed. The Canadian essayists, dealing with familiar and important topics, often ignore or talk past their American critics. The American contributors, on the other hand, are limited by the content of the principal essays, and generally venture little more than a short rebuttal or critique.

A further criticism which might be advanced has to do with the juxtaposition of the various points of view on the key issue areas. While many writers make the case for Canadian federalism, only René Levesque presents the separatist position. Disappointingly, however, his essay suggests the "soft-sell" intended for American audiences and contains none of the passion of the Independentiste. The same might be said of Walter Gordon's contribution on Canadian independence vis-à-vis the U.S. Gordon's mission, like Levesque's, comes across as essentially diplomatic rather than either polemical or analytic. Willis Armstrong, in his critique of Gordon's contribution, does not make the case for freer trade between the U.S. and Canada or for such ideas as a North American Common Market. Hence, as in the case of the Quebec independence issue, the debate sought by the editors is in a sense never really joined.

These critical observations however must be tempered by an acknowledgement of the strengths of the book, of which there are many. The seminar assembled an impressive roster of participants from both sides of the border. Mordecai Richler and Sylvia Wright discuss Canadian national identity; Frank Peers and Joel Rosenbloom examine communications policy, one of the most complex areas in current Canada-U.S. relations; Ian McDougall and former U.S. Interior Secretary Stewart Udall together with Robert Stein examine energy and natural resources policy. All of these are strong and well-written contributions to the volume. Equally valuable are the contributions of Richard Simeon on the future of federal-provincial relations in Canada and the essay by Peter Gourevitch which attempts to place Quebec separatism in a comparative context by drawing in parallels to nationalist movements in a number of European countries. Prominent Canadian political figures such as Flora MacDonald, Robert Bourassa, John Roberts, and Gordon Gibson are represented among the contributors, as are well-known academics such as Robert Art, Michael Parenti, Walter Dean Burnham, and François-Pierre Gingras. Their contributions individually reflect the high quality and thoughtfulness that one would expect from a distinguished list of participants. The concluding essay by co-editor Neil Nevitte is more than the obligatory reiteration of the main themes of the contributors. It is a serious and well-written overview of the larger theme of "Nationalism, States, and Nations."

While the book ultimately fails to achieve some of its more ambitious goals, many of the individual essays will be read with interest by students of contemporary Canadian politics and economics on both sides of the border. However, the reader does not acquire a sense of the rich academic literature which has developed around these topics in Canada, although this weakness is partly redressed by the inclusion of select bibliographies on Canada-U.S. relations, Canadian federalism, and Quebec nationalism. And, as Simeon notes, "The pace of political debate and the level of conflict on constitutional change" have increased significantly since this 1977–78 seminar, thereby rendering the task of the editors even more difficult. Nevertheless, the American reader who wants to understand the main currents of nationalist and federalist thought in contemporary Canada will find this collection a very useful way to begin.

LAWRENCE LEDUC

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War Will Not Take Place: The French Parliamentary Elections, March 1978. By J. R. Frears and Jean-Luc Parodi. (New York: Holmes and Meier, 1979. Pp. x + 147. $17.50.)

This short book by two political scientists, one from England and the other from France, both with solid credentials as analysts of French politics, draws its title and central theme from Jean Giraudoux's play, "The Trojan War Will Not Take Place." For Giraudoux, that war could have been avoided had Trojan and Greek leaders exercised better sense. Frears and Parodi see a French parallel: "There is nothing in contemporary French society, no rooted economic discontent, no implacable separatist loyalties to church or region, no oppressed minority, which allows one to regard conflict as inevitable" (p. 1). Yet the legislative elections of 1978 were presented to the voter as a dramatic *choix de société*.

The reader may suspect that we are to hear, once again, that France is a land of solid, conservative citizens led by highly ideological, quarrelsome politicians. Instead, we are treated to a perceptive and well-documented analysis of strategies and constraints which lay behind this most recent frustration of the French left. In support of their central theme of declining conflict in French politics, the authors note the increase of Socialist strength in traditionally conservative, Catholic regions, where church attendance is declining, the very limited electoral support for the extreme left and the extreme right (3.1 percent and .5 percent, respectively, on the first ballot), and the rise of the two most "moderate" parties–Giscard's UDF and the Socialists–at the expense of the Gaullists and the Communists.

Among several significant subthemes, the following are particularly noteworthy. Frears and Parodi trace the trend toward bipolarism in French politics partly to the success of Gaullism, but also to the direct presidential election, which forces coalitions, at least in the runoff, and advantages the party led by "a credible Presidential candidate" (p. 17). Like several recent studies by American scholars, this one challenges the notion that France is a land of extreme political centralization. The authors stress the importance of *local* campaigns and of *implantation locale.* Eighty-seven percent of successful candidates in 1978 were either elected local officials or incumbents, five-sixths of whom also held local office. The authors give careful attention to what was perhaps the most striking feature of this election: the rivalry between coalition partners. They attribute the quarrel within the majority to a simple struggle for power: "On the right such wars as there are, like those between animals or birds or the Mafia, are more about territory than belief" (p. 23). "Like a faction in Japanese politics, the RPR is the instrument of the ambition of Jacques Chirac and the hopes of patronage that would flow from its success" (p. 25). This interpretation, while fundamentally correct, gives too scant attention to lingering loyalties among RPR militants and supporters to the legacy of de Gaulle and his *certaine idée de la France.*

As for the opposition, Frears and Parodi accept the widely held view that "The left . . . lost because the Communist Party judged that it was less important to win the election than to avoid being eclipsed by the Socialist Party" (p. 114). The bitter dispute between the two parties apparently reinforced "the trend of the 1970s whereby the electorate always vote for the left except when the left might win power" (p. 118). Repeated pre-election polls found the left 2 to 6 percent ahead in expressed voting intentions, but marginally behind in a tally of whom respondents would like to see win "in your heart of hearts" (p. 29). Yet, as the authors argue persuasively, the constraints of the electoral system and the dependence of the Socialists' success upon their identification with the left opposition leave them no good alternative to the Communist alliance.

There seems little doubt that the intensity of political conflict has diminished and that support for existing political institutions has increased in France since the 1950s. Frears and Parodi are correct in arguing that a victory of the left likely would have been short-lived and would not have produced any drastic changes in the political system. Yet they sometimes overstate the new consensus, although less blatantly than in some of Frears' earlier work. Short of massive political violence, the intensity of social and political conflict is difficult to measure and more difficult to compare from country to country. Before concluding that France is now a "consensual" society, one needs to bear in mind the evidence of continuing tension. France remains a society in which a fifth of the electorate is loyal to a Communist party that remains ambivalent about shaking off the remnants of its Stalinist past, a society in which the left gets 88 percent of the votes of those without religion, but only 15 percent of those of regular churchgoers, a society which demonstrated the fragility of government authority in the events of May, 1968, a society which (as Frears and Parodi acknowledge) has seen no alternation in governing majorities under its present constitution, and, perhaps most important, a society in which there is widespread popular resentment against a distribution of income that is among the most unequal in Europe. The present French trend toward class voting is unlikely to lead to social harmony, Swedish-style, especially if the present recession worsens. Support for French political institutions increased not simply because French society is more "consensual" but because citizens perceived that in general the institutions were performing well. He who sees more harmony ahead should be aware that perceptions change.

The political scientist might wish for a closer examination of the French experience in the context of the general literature in comparative politics, for example, on the relationship between electoral laws and party systems, on class voting, and on the catch-all party. Yet this is an excellent and insightful account of a French election in which allied parties in both camps found themselves–and will find themselves–condemned to live together or to renounce any hope of victory.

JOHN S. AMBLER

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Political Participation in the USSR. By Theodore H. Friedgut. (Princeton, N.J.: Princeton University Press, 1979. Pp. xv + 353. $18.50.)

Probably there is no more troublesome concept in communist studies today than that of "participation." Among scholars of communist political systems, Jerry F. Hough in particular has challenged the field to employ the term in a way sufficiently inclusive to invite comparative behavioral research; such a conceptualization then yields strikingly high figures for participation in public activity by Soviet citizens, while questions as to the meaningfulness of such participation may be ruled out on methodological grounds.

The glove thrown down by Hough and others has been picked up by this able work of Theodore H. Friedgut. It operates, with somewhat varying effectiveness, on three levels. It begins with and develops throughout a definition of the concept of participation. Friedgut occupies a middle position between those denying political importance to the formalistic and ceremonial ways in which a great part of the adult Soviet population responds to pressures for social and political service, and those who erase the distinctions between such activity and the public acts of citizens in polyarchies. He justifies this with the argument that the restrictive position, confining the study of participation to the influence on "inputs" into policy making, needlessly excludes from attention the study of variations in the effect of participation on administrative "outputs." His discussion of the concept is consistently thoughtful and nuanced, and his conclusions on this score judicious.

Where he seeks to connect his discussion of participation to a yet more general level of analysis, the dynamic perspective of the "modernization" of Soviet society, his work is less focused. One senses a strong underlying theory of modernization, but its premises are offered piecemeal. He calls the Soviet Union a "transitional" society and closely follows Huntington's criteria of modernization, without pausing over the pathologies Huntington has outlined. Frequently one wonders whether the course of Soviet modernization is as inexorable, and its social, economic, and political correlates as closely coupled, as some of Friedgut's statements imply. Theoretical disputes over development are far from settled, but very few writers would resort to the naive ascription of entelechy to development of an assertion, for example, that the continued obstacles to the merging of state and civil society in the USSR owe to "lingering immaturities of the Soviet political system" (p. 314). By what yardstick can one determine that the system today is not fully mature? At this level the book might have benefited from some consideration of the literature on stable authoritarian systems.

However, at the principal level of the book the author more than fulfills the reader's expectations. Friedgut explains in close detail the network of institutions in which the citizenry engages in public participation. These are clearly categorized and their effectiveness well analyzed. Friedgut does not stray from examining local-level participation, excluding the party, Komsomol, trade unions, and national voluntary societies from his study. Instead he examines local soviets and *ispolkomy* along with their committees, commissions and departments, as well as the task-oriented groups in which deputies and citizens solve local problems. An especially valuable finding in the work concerns the emergence of a "core" of public activists, a category closely coinciding with the membership of the party. These activists typically take on a multiplicity of voluntary assignments, while among the "periphery" of ordinary citizens, such participation is far more modest and indeed frequently is only nominal.

Friedgut's research is based on an exemplary mixture of published Soviet materials, his own field observations in the Soviet Union, and interviews with recent emigrants. Reconciling the incompatible views of Soviet reality that emerge from these disparate sources has bedeviled most people in the field, but Friedgut has handled the contradictions with admirable skill, setting incommensurate views against a broader framework. The result is a reasoned and consistent conclusion that local participation may often serve as an effective supplement or alternative to standing administrative agencies but nowhere escapes the close and personalized controls that set the limits of participation's forms and content.

Some minor errors, particularly in the transcription of Russian words (correct forms: la*v*ke, p. 315; Amal*r*ik, p. 295; Zhili*shch*no-, pp. xv and 206; uchastk*ovyi*, p. 204) detract slightly from this excellent book.

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Land Tenure and Peasant in South Asia. Edited by Robert Eric Frykenberg. (New Delhi: Orient Longman, by arrangement with the Land Tenure Center, University of Wisconsin, 1977. Pp. x + 312. Price not given.)

Studies of South Asia which begin by alerting the reader to the infinite variety, the complexity, and the immensity of the subcontinent and, thus, of the research problems under investigation, are sometimes also cueing the reader to an additional message. "Don't expect," it may be interpolated, "that this book will provide you with a tidy set of questions and answers. The purpose of this material is to enrich, not economize, to explore the byways, not map the terrain."

This collection of 13 essays on land tenure in South Asia is such a volume. It contains uniformly fine essays that address the relationship between "land, lord, and labor." The essays illustrate the widely varying forms of land tenure and provide different perspectives on how land tenure arrangements have affected and have been affected by the ideologies and institutions of precolonial, colonial and postcolonial periods.

The essays are rich with ideas and innovative theses. At the same time, the book is frustrating because the themes and questions raised by individual essays are insufficiently connected from one chapter to the next and are not fully integrated in the editor's introduction. Because there is no discussion in the introduction of the individual essays but only a mention of general themes (the ineffectiveness of socioeconomic engineering; the greater importance of vertical over horizontal linkages; the inconspicuousness of the rural poor), the reader is left alone to ferret out the relevant material on those and other issues from the abundance of data. Although the reading of the essays as an integrated work on the problem of land tenure is not made easy, they *are* easily read for their individual value.

The first section of the book, "landed privilege in the past," includes an important essay by John McLane which argues that the Cornwallis land policy of 1793 (the Permanent Settlement) and its consequences are traceable in part to a pattern of earlier changes in which large landowners (*zamindars*) had already begun to cede control to intermediaries and village headmen. Three subsequent and extremely fine essays by Robert Frykenberg, Eric Stokes and Burton Stein *do* address a common theme, debating the role which *inam* ("privileged" tenure or the "silent settlement") played in local hierarchical relations and in the establishment of British authority in South India.

The second part of the book is a much looser grouping of essays on "dimensions of landed relations." It includes one essay by geographer Brian Murton on the agro-ecosystem of Salem, Tamil Nadu in the late eighteenth century; a second by historian Michael Adas, on the peasant (*ryotwari*) system of tenure in lower Burma in the nineteenth century; and a third by political scientist Ronald Herring on credit-capital and land tenure in contemporary India.

Part 3 on "Dimensions of Development and/or Reform" is the section of the book likely to be of greatest interest to readers of this *Review*. In an essay on the contributions of anthropology to the study and practice of development, Paul Hiebert concludes on the sound but not startling note that a program's effectiveness will depend on how incentives are conceptualized within the world view of those involved and on the extent to which the total interlocking cultural matrix has been understood. Carl Gotsch analyzes the interdependent relationship between land tenure differences and policy decisions regarding agricultural mechanization in West Punjab (Pakistan) and East Punjab (India). Joseph Elder's essay does an admirable job of sorting out the different schools of argument on the question of the relationship between India's secular and sacred hierarchies and argues that economic landed rights are critical to the success of low-caste attempts to rise in the sacred hierarchy. Tom Januzzi's essay is a detailed, careful analysis of the failure of agrarian reform efforts in Bihar. It would have been all the more interesting had the essay endeavored explicitly to compare Bihar's experience with other states and account for the less successful land reform efforts in Bihar, particularly since the more effective efforts of Maharashtra and Kerala are the subject of the final essay by Henry Hart and Ronald Herring.

This last essay will be perhaps the most exciting in the volume for students of contemporary Indian politics. In a dense and interesting analysis, the authors relate the historical connection between nationalism and landed elites, the early reform efforts, class-party alliances, and the differing commitment of Kerala and Maharashtra to redistributive reforms in the late 1960s. Although the different routes to and records of land distribution in the two states are striking, the consequences of land reform for at least one class, the rural poor, should not, as the authors caution, be overdrawn. While reform efforts in the more radical context of Kerala have improved the position of the renter and those even more economically marginal, the limits of reform are clear. Redistribution of all above-ceiling land to the individual tiller would not be sufficient to meet the minimum survival needs of Kerala's rural population (p. 243).

As Robert Frykenberg observes in his introduction, "[Those] on the very brink of extinction [seem] to appear more and more fading and ghostly. . . . For almost all intents and purposes, the better-off seem oblivious to those who lie in the dust. This obliviousness is not so much a deliberate and studied matter . . . as it is a kind of subconscious repression of facts and a sense of futility" (pp. 13–14). This book moves us a step forward towards raising some of those facts to the level of the conscious. It portrays one fact of the structural arrangements in rural society documenting the emergence of a powerful owner-cultivator class alongside or in succession to the larger landlord class. But the other face of power relations in the agricultural sector is less closely depicted. Who exactly are the near-landless and the landless in the Punjab, in Maharashtra, in Kerala? How, if at all, are they affected by mechanization, land tenure changes, union organization, public works programs, credit arrangements, rural industry, and broadly targeted educational and health programs? This volume begins to address some of these questions, the Hart-Herring essay most prominently. However, to make fully visible that class too often overlooked in the study of South Asian rural society will require more extensive work in subsequent volumes.

MARY FAINSOD KATZENSTEIN

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Crises of Political Development in Europe and the United States. Edited by Raymond Grew. Social Science Research Council Committee on Comparative Politics, Studies in Political Development 9. (Princeton, N.J.: Princeton University Press, 1978. Pp. xi + 434. $27.50, cloth; $6.95, paper.)

Development shows common critical phases but in no common sequence among 16 otherwise commonly "Western" nations in this comparative chronologue (Pye, p. viii). It shows no modal ("ideal") sequence of identity > legitimacy > participation > penetration > distribution crises—just conditionally modal "priorities" or starting points (pp. 33, 216, 343). Thus: with earlier or insular conditions, legitimacy/participation are prior as in America (Hollingsworth), Belgium? (Zolberg), Britain (Thomas) and Scandinavia (Dovring). With opposite conditions, penetration is prior as in Germany (Gillis), Poland (Szporluk) and Russia (Pintner). With intermediate conditions come mixed priorities among France (Bien and Grew), Italy (Grew), Portugal? and Spain (Payne). Yet this diversity implies some negative uniformities; e.g., development seldom starts from distributive or simultaneously timed crises (pp. 24, 27). The argument is eclectic, drawing its framework from political science and analysis from political historiography.

The *framework* links the crises within to the direction of development toward "increased capacity, equality, and differentiation" (p. 6). But this triad resists measurement (p. 9) and its own sequencing may confound the crisis-sequence, especially when both are historically accelerated and causally complex. Indeed, "sequence" and "direction" seem tautological, depicting the same process via discrete events (e.g., "participation" crisis) versus continuous dimensions (e.g., growing "equality") respectively. More promising is the framework's versatile dynamics: it couples cyclical with novel effects via recurrence of old crises like "national identity" in new forms like "ideological identity" (p. 17). It surpasses conventional serial/stage models by linking all, not just proximate, crises in the sequence (p. 32). Its spectrum of "early" to "late" developers (p. 3) implies sequencing not only of crises per country but of countries themselves; such a macro-sequence might signify cumulative (e.g., arithmetic) progression effects upon ever later countries.

The *design* appears inverted—mapping a framework with no proposition inventory to frame onto data with no data/measurement theory (p. 12) such as "acute crisis analysis" from international relations. It appraises the framework on polity "development" from volume 7, but not on polity "formation" from volume 8, thus precluding the overview needed for this reportedly final volume in the SSRC series. It lacks rules to separate developmental crises from other crises (e.g., America's Great Depression?) or from each other (cf. Russia's Time of Troubles) and to sequence crises that persist or overlap (pp. 10–12). Still, such a multi-longitudinal design could compare "spontaneous" (legitimacy/participation-based) and "forced" (penetration-based) maturation across time (pp. viii, 343) with perhaps analogously spontaneous (spill-over) and forced (imperial) diffusion of crises across nations; it might even integrate such processes *qua* global development theory.

The *sample* of European-settled polities, including late-comers like Germany and Poland, enjoyed pre-national development via ecclesiastic identity, legitimacy and penetration (pp. 18–22)—unlike the modest initial conditions of today's developers. It may thus exaggerate "preformist" resurgence of prior phases, over "epigenesis" of new phases where nation building starts from scratch. It shuns end-state conditions (pp. vi, 15) of "developed" polities, ignoring how theory can logically encompass such an open-ended process—a seemingly inductive fallacy. It cannot randomize error with Europe's homogeneity (p. 4) nor equalize error with that homogeneity imperfect, thus urging a global sample. Happily, this selection covers the same polities as protonational (e.g., Vol. 8) and supranational (EEC-oriented) surveys of Europe, together affording a "panel study" of its development. And it outspans generational and cyclical effects (pp. 192, 213), albeit redeveloping antiquarians like China challenge the significance of long time-frames.

The *findings* tie crises to neither the origin nor outcome of development (pp. 94, 317) unless reductively defined as the first and last crises themselves. They tie crises temporally, not causally (pp. 44, 308), thus daunting even a reductive account of development. Nor do they anticipate future crisis categories (like post-ideological identity, or post-economic distribution?) despite past crisis resolutions and future "cultural vitality" (p. 35). And they address no other sequential models—even when drawn from the same sample (p. 34n)—thus slighting their explanatory tradeoff with the crisis perspective. The findings do revise that perspective from an original "ideal" sequence fitting earlier, insular (read: innovative?) developers alone (Britain and Scandinavia, pp. 29, 91) to contingent "priorities" covering all developers; but this sacrifices significance for scope, since priorities are more vulnerable than whole sequences to chance ($p = .20, .01$, respectively), admittedly making the revision a Pyrrhic theory.

The work's broader *import* extends to revolutionary theory by distinguishing "revolution" from "development," namely, as simultaneously timed versus sequentially resolved crises (pp. 265, 301), thus de-tautologizing their association amid misnomers like "industrial revolution." It affects foreign policy pending externalization of crises like penetration (imperialism), distribution (foreign aid) or identity (regional integration). It challenges political economy by denying priority to economic-distributive crises in polity building (pp. 159, 212). It also lends classrooms a readable country-by-crisis casebook. Still, it avows no development theory (pp. 8, 34) or even hard evidence of its feasibility, despite the book's very purpose. It vexes development policy by neglecting functional equivalents to the sequentially spaced crises so crucial yet obsolete within the newer states' telescoped time frames. Overall, this opus offers no coda to the committee's étude in theory-composition; perhaps like the Yellow River Concerto, it illumines the problem of creation-by-committee.

CRAIG MCCAUGHRIN

University of Pennsylvania

The Ostpolitik of the Federal Republic of Germany. By William E. Griffith. (Cambridge, Mass.: MIT Press, 1978. Pp. 325. $25.00.)

During the first two decades of the Federal Republic of Germany, from 1949 to the late 1960s, West German foreign policy was characterized by the sharp contrast between the failure of Bonn's reunification policy and the remarkable successes the Federal Republic achieved in security policy and political and economic recovery. This contrast developed from the impossibility of pursuing these goals simultaneously—first in a cold-war setting, and then in the context of an uneasy phase of coexistence—because the cross-purposes and power relationships between East and West were sharply polarized. The very success of Bonn's pro-Western security policy and recovery policy, through which the Federal Republic became the bulwark of Washington's containment policy in Europe, solidified the cold-war alliances in Central Europe and further deepened the division of Germany.

It was clear to the Bonn government from the beginning that in contrast to other West German foreign policy goals, the unification of Germany required at least the acquiescence, if not the direct sponsorship, of both cold-war blocs. Consequently, the Adenauer government's long-range reunification strategy was based on two central tenets: (1) that Washington and Moscow held the key to the German question, and (2) that with the passage of time the balance of power between the cold-war blocs would shift in favor of the West, thus allowing negotiations "on the basis of strength" which would induce the Soviet Union to settle the German question on Western terms. The first of these tenets (which was essentially correct) clearly required, above all else, a solidification of the German position within the Western alliance. Because of West Germany's relative weakness, the most immediate aim of Bonn's unification policy was to enlist the active support of the Western powers, especially the United States, and to ensure that the West would not treat the German question as a secondary issue that could conceivably be traded off in an overall American-Soviet settlement of the cold war. These objectives were based on the realization that the Western powers would view the prospect of a unified Germany with apprehension. Thus, the Bonn government's unification policy was to be implemented by increasing the political leverage of Bonn within the Western alliance in order to solidify on the political plane the legal and moral commitment of the Western powers to support reunification and to regard the Bonn government as the only legitimate spokesman for all Germany.

The complementary aspect of this policy—oriented toward Moscow—was much more passive and vague, because it was merely an appendage of Bonn's Washington-oriented policy. This was perhaps unavoidable. The Western powers most likely would have viewed German overtures to the Soviet Union with misgivings, and a much more active Eastern policy would have jeopardized the entire treaty structure that was to restore sovereignty to the Federal Republic. This would have undermined the power base from which Adenauer expected to deal with the Soviet Union at some future date, not to speak of the risks Bonn would have incurred in dealing with Moscow independently. As a consequence, Bonn's Eastern policy inevitably appeared flaccid and unimaginative, especially in contrast to the political acumen and tenacity displayed by Adenauer in his dealings with the Western powers.

For a variety of reasons, the erosion of bipolarity during the late 1950s and throughout the 1960s was hardly more propitious for the cause of unification. As the Soviet Union developed a formidable nuclear arsenal, it became increasingly unlikely that the Soviet Union could be pressured to allow a change in the political and territorial status quo in Central and Eastern Europe. Moreover, Bonn's Western allies found the status quo equally tolerable, and began to seek an accommodation with the Soviet Union based on the existing state of affairs in Europe. On the whole, the major unification efforts made by the Bonn government from the middle fifties until the late sixties were not really directed toward bringing about unification on terms acceptable to Bonn—for that there was little hope—but rather sought to prevent the legitimization of the status quo in Central Europe. The "policy of movement" initiated by Bonn in the middle 1960s, which was designed to create an "opening to the East," faced formidable obstacles, some of which were self-imposed. The Bonn government seemed insensitive to the fact that declarations of good intentions meant little to Eastern Europe when they were coupled with implicit demands for territorial revisions. Moreover, the East European governments—which after all were amicably disposed toward East Germany and highly sensitive to the policy guidelines set forth by the Kremlin—could hardly endorse Bonn's attempt to isolate the East German regime, an attempt that remained an essential part of the Federal Republic's new Eastern policy.

Soon after Chancellor Willy Brandt's coalition government took office in October 1969, it became clear that the new Bonn government intended to pursue a dynamic and innovative policy toward the East. This new policy—generally called Ostpolitik—reflected the recognition that a dynamic Eastern policy would require that Bonn deal in a forthright and realistic manner not only with Moscow and the East European capitals, but also with East Berlin. In other words, the Brandt government was willing to do its part in legitimizing the territorial status quo in Central Europe and in bringing West German foreign policy in line with the explicit desires of the Eastern countries as well as with the tacit assumptions and policies of its Western allies.

William Griffith tells the story of Ostpolitik clearly and responsibly. One might disagree with him over points of interpretation and emphasis, but his account is reasonably argued and by and large persuasive. Nonetheless, the reader experiences a certain dissatisfaction and disappointment. Especially in the first two-thirds of the book, where the narrative takes us from World War I to the interwar period and through World War II to the 1950s and 1960s, I began to suspect that my agreeable attitude stemmed in good part from the fact that much of Griffith's argument was so familiar—much of what he has to say has been said before and, in some cases, has been said better. When the narrative begins to deal with the Ostpolitik of the 1970s, a subject as yet less ploughed over in the English-language literature, one wishes that Griffith had devoted more than a mere 50 pages to Willy Brandt's Eastern policy and its significance for the European political order. In that part of the book, where one anticipates a searching assessment of the implications of Ostpolitik and its future prospects, the reader—and especially the specialist—feels let down by an excessive analytical restraint that appears content to let "facts" speak for themselves. This is especially regrettable since the bulk of the book—the pages that precede the discussion of Brandt's Ostpolitik—offers so little in the way of reinterpretation and seems intended primarily to provide a background for the issues of the 1970s. In that respect, Griffith's first two sentences appear somewhat disarming and ingenuous: "This is a study in contemporary history, not in political science or sociology. I neither hypothesize, test, nor predict; rather, I have tried, while knowing that I could not wholly succeed, to 'tell it as it really was.' " It is true, of course, that analysts of contemporary history are allowed to be imaginative only in matters of form, not facts. But imagination applied to form can be considerable, and Griffith would have given us a better book had he been less reluctant to exercise it.

WOLFRAM F. HANRIEDER

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Die Wahlen in den Schweizerischen Nationalrat 1848–1919: Wahlrecht, Wahlsystem, Wahlbeteiligung, Verhalten von Wählern und Parteien, Wahlthemen und Wahlkämpfe; Les Elections au Conseil national suisse 1848–1919: Droit et système électoral, Participation au scrutin, Comportement de l'électorat et des partis, Thèmes dominants et données majeures des campagnes électorales, 3 vols. By Erich Gruner et al. (Berne: Francke Verlag, 1978. Pp. 1189 + 300 + 540. Price not given.)

Contemporary Swiss politics is well known for its strong commitment to the principle of proportionality. The lower house of the federal legislature, the National Council, is elected by proportional representation, and the federal executive is composed according to the famous "magic formula" which gives each of the four principal political parties a proportional share of executive power. In both respects, the period from 1848, the year of the adoption of the federal constitution, until 1919 presents a sharp contrast: this was the era of Radical party dominance and of majority-plurality elections. The electoral system prescribed mainly multimember constituencies in which the candidates required majorities in order to win on the first or second ballot, but only pluralities on the third ballot; from 1900 on, only two ballots were held.

The National Council elections during the 1848–1919 period are the subject of this monumental and extremely impressive research effort by Erich Gruner and his team of collaborators. Their basic data are presented in a series of tables organized by election and constituency (Vol. 3, pp. 7–341). The following information is provided: the exact date of the vote, the kind of election (general or special elections, and first, second, or third ballot), the number of members to be elected in the constituency, the number of registered voters, the number and percentage of actual voters, and the names, occupations, places of residence, numbers and percentages of votes received, party memberships, and party endorsements of all candidates. In addition, there are a series of summary tables on electoral participation, party strengths, successful and unsuccessful candidates, etc. (Vol. 3, pp. 343–496), and a set of maps and graphs (Vol. 3, pp. 497–535).

The first volume consists of more than 1100 pages of background information, explanations, and analysis, as well as a thorough index. For instance, the electoral law and questions of apportionment are treated at length (Vol. 1, pp. 94–160, 312–78). The longest chapter discusses the issues and campaigns of each election (Vol. 1, pp. 603–918). Most of the text is in German, but a few sections are in French—in particular, the discussion of election issues and campaigns in French-speaking Switzerland (Vol. 1, pp. 800–918), and an overall summary (Vol. 1, pp. 1081–1109). The second volume consists of 300 pages of detailed notes.

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Directing Technology: Policies for Promotion and Control. Edited by Ron Johnston and Philip Gummett. (New York: St. Martin's Press, 1979. Pp. 271. $22.50.)

The concept of science policy emerged in the aftermath of World War II as a way of analyzing government strategies aimed at the advance of a scientific knowledge and technological innovation. A cursory review of the literature associated with that concept suggests that technology has been viewed largely as a by-product of scientific advance. Science policy analysts, it appears, have accepted the contention of the scientific community that the road to technological innovation must pass through basic research and that sound government policy must emphasize advances in the understanding of nature. The argument may be accurate, but it is not disinterested; indeed, the principal representatives for science *are* the principal science policy analysts in the United States, and many of their studies are little more than expressions of preference masquerading as analysis.

One unfortunate consequence of the dominant role of scientists in the science policy community is a paucity of studies focusing on government efforts to promote the development and deployment of technologies. Technology itself has not been ignored by all scholars, particularly in recent years, as innovation has become more controversial. Disputes over nuclear power plants, airports, and other large technology-based facilities have focused attention on the detrimental impact of technology and elicited new interest in its regulation and control. But if controversies over technological development have piqued the interest of philosophers, sociologists, and even some political scientists, they have had little impact on students of science policy.

Directing Technology, a collection of 13 essays, is not simply another reader on science policy, precisely because it is concerned with government efforts to direct technology. The volume is divided into two parts, reflecting the editors' definition of "direction" as both promotion and regulation. Part 1 includes essays on such topics as government support for industrial innovation (Keith Pavitt), the development of nuclear technology (Roger Williams), and technology policy and the motor industry (Peter Stills). Essays on the control of technology through regulation (Judith Reppy), the role of participation in technology assessment (Michael Gibbons), and British safety policy (Brendan Gillespie) highlight part 2 of the collection.

American political scientists concerned with the direction of technology will find this collection useful, for the essays focus largely on British policies. The experiences of other advanced industrial states must be analyzed and explicit comparisons drawn if a theory of technology policy is to be developed. Several contributions to this volume, such as Philip Gummet's essay on the application of the contract mechanism in Great Britain and Edward Yoxen's account of the regulation of recombinant genetics, are particularly useful in light of comparable materials available in the American literature.

Viewed from a different perspective, however, this collection is disappointing. The majority of the essays are overviews of government policies with respect to technology. Overviews, unfortunately, are the curse of technology studies; any significant advance in our understanding of the relationship between government and technology requires the development of detailed accounts of specific actions and decision-making practices. There are, of course, a few case studies in the volume, notably John Hartland's and Michael Gibbon's analysis of government involvement in the development of technology for aircraft landing systems, but the editors apparently preferred to emphasize broad strategic analyses rather than detailed cases.

There are other difficulties as well. The essays vary considerably in quality; some are analytic and quite provocative, others are largely descriptive and rather dull, and still others are replete with technical jargon and virtually unreadable. Moreover, the editors do not provide a coherent framework for the individual contributions in this collection. The distinction between promotion and regulation, which they develop in a short introduction, is insufficient to justify the precise selection of essays. But these flaws do not detract from the primary virtue of *Directing Technology,* i.e., its focus on technology rather than science in the analysis of government policies. The appearance of this volume, consequently, is noteworthy, and *Directing Technology* is a welcome addition to the literature on science policy.

JEROME E. MILCH

University of Pittsburgh

Sri Lanka: Third World Democracy. By James Jupp. (Totowa, N.J.: Frank Cass, 1978. Pp. xxi + 423. $29.50.)

This narrative political history is the most comprehensive and detailed study of politics in Sri Lanka in print. Exhaustively researched and well-written, the study demonstrates Jupp's impressive skills in synthesizing the existing literature on the politics of this small South Asian nation. Although not evidently aimed at the area-specialist audience, because it is a single case study in the tradition of British and Commonwealth scholarship, and because nonspecialists are apt to find the detail overwhelming, the most appreciative readers are likely to be students of Sri Lanka and of South Asian politics.

The main strengths of this study lie in the descriptive rather than the analytical realm, not because it is a case study of a single country but because the author has been little moved by the intellectual revolution in the study of comparative politics. Jupp implicitly rejects the concepts and methods of modern social science and makes no pretense of facilitating any systematic comparison of Sri Lanka with other states. If judged by its analytical successes, this book would have to receive average marks. Jupp's interpretations and conclusions may be correct, but often they are not systematically supported or convincingly demonstrated. Take, for example, his observation that Sri Lanka is Asia's "most effective welfare state" (p. 29), "only effective welfare state" (p. 248), and "most elaborate welfare state" (p. 342). Propositions of this nature call at least for a definition of a welfare state, some criteria for determining effectiveness, and indicators that would permit measurement and cross-national comparison. Moreover, such propositions would seem to require data comparing welfare state outputs in Sri Lanka and selected other Asian states. In the absence of these essentials, Jupp's enthusiastic claims have to be accepted on faith alone. The author's occasional comparisons, all of which are favorable to Sri Lanka, are not limited to Asian or other Third World neighbors. In his view, Sri Lanka is "every bit as modern in terms of political institutions and practices as the great majority of advanced industrial nations" (p. 349), and Sri Lanka's political system "provides a better model of participatory democracy than many states of Europe or America" (p. 327).

If points such as these suggest that Jupp is not always dispassionate, it is not an entirely uncritical account. Sri Lanka's very considerable success with democratic institutions and the large measure of political stability it has enjoyed are discussed with the enthusiasm of one who is obviously very impressed with these accomplishments. While mention is made of problem areas, particularly religious and ethnic communalism and the economy, these seem less troublesome to Jupp than to most other students of Sri Lanka. In accentuating the positive in this upbeat account, Jupp is open to the charge that the political implications of a near-stagnant economy and seemingly intractable Sinhalese-Tamil tensions are not portrayed in terms of their apparent importance. According to most accounts, Sri Lanka's strides in welfare have been in large measure accomplished by consuming its output, leaving little available for the capital investment needed for economic expansion. Whether Sri Lanka's commitment to democratic socialism and the welfare state can be sustained within the framework of a Third World democracy has to be closely related to its ability to cope with increasingly severe economic development problems.

These criticisms notwithstanding, this is a valuable study, and the patient reader will gain a great deal from it. It is regrettable that the price is a virtually prohibitive $29.50.

GEORGE H. GADBOIS, JR.

University of Kentucky

Uruguay in Transition: From Civilian to Military Rule. By Edy Kaufman. (New Brunswick, N.J.: Transaction Books, 1979. Pp. xv + 126. $9.95.)

The demise of civilian politics and individual liberties in Uruguay has failed to capture the interest of scholars (or, for that matter, of the general public) as the same experience in Chile has done. Uruguay's democratic traditions were deeply rooted in its history, and military intervention would have seemed unimaginable a generation ago. The events in Chile were swift and dramatic, but in Uruguay the military takeover was gradual, literally years in the making, and all but invited by a despotically inclined civilian president; the author refers to the takeover as an *autogolpe* (self-coup). The sequence of events in Uruguay are important and interesting in themselves, but the very gradualism of the experience also provides an unusual opportunity to analyze some of the conditions that promote military intervention; in the case of Uruguay, these conditions were sustained economic decline, urban terrorism, corruption, and institutional paralysis.

Edy Kaufman clearly is aware of this opportunity, and takes good advantage of it, despite his self-acknowledged emotional involvement in the fate of the country. There are no particular surprises in the book, but there is a great deal of careful, persuasive analysis and attention to detail. The book contains extensive and useful notes, as well as a methodical reconstruction (in the appendix) of the sequence of events which spans nearly a decade, preceding, during, and following the military takeover.

The major fault of the book, and it is a relatively benign one, is Kaufman's unfortunate choice of a conceptual framework originally designed for the analysis of foreign policy decision making, which is both awkward and inappropriate, and confuses the organization and detracts from the cohesion of his analysis. Toward the end of the book there is a poorly titled chapter, "Testing Hypotheses," which tries to compare the Uruguayan experience with some 23 propositions culled from the literature on military intervention in Latin America as well as two propositions which

Kaufman has extracted from his own study and suggests might be applied to Chile.

These flaws are not, however, critical. The book is a useful source of information and analysis on recent politics in Uruguay, and that country's unusual experience with military intervention.

RONALD MCDONALD

Syracuse University

Economic Response: Comparative Studies in Trade, Finance, and Growth. By Charles P. Kindleberger. (Cambridge, Mass.: Harvard University Press, 1978. Pp. vii + 308. $16.50.)

Social scientists have recently taken note of the abundance of historical materials available for generating theories of social change. In *Economic Response,* Charles P. Kindleberger draws on his extensive knowledge of nineteenth-century Western European and North American economic history, and demonstrates the utility of comparative historical economics, an approach which the author argues "allows us to test a given economic model and to see how general and how special it is by examining the same process or stimulus under other conditions" (p. 2). The first and last chapters address issues of method; the six remaining chapters—of which five have previously appeared as articles—are substantive in nature.

Of particular interest to political scientists is Kindleberger's willingness to venture beyond economic analysis, and offer social or political explanations for diverse responses of policy makers to international economic developments. For instance, in his classic essay entitled "Group Behavior and International Trade," Kindleberger examines five national reactions to the international decline in the price of grain after 1870. Given standard economic assumptions, there should have been a general increase in extra-European imports; Kindleberger demonstrates, instead, that German and French policy makers imposed agricultural tariffs, the Danish peasantry modernized their agricultural methods and marketing procedures, and Italian politicians failed to provide either aid or protection for the peasantry, thereby contributing to the increasing overseas migration. Only in Great Britain did a free trade policy result in further non-agrarian specialization. Kindleberger attributes these diverse responses to the interaction of interest groups within each nation-state. He then concludes with a call for "an adequate theory of the behavior of large groups . . . as an adjunct to the analytical tools of the market" (p. 38). Similarly, in "The Rise of Free Trade in Western Europe, 1820 to 1875," Kindleberger cites economic as well as non-economic causes for the adoption of free trade policies among Western European nations: dominant domestic groups expected to benefit, industrialists sought continued access to raw materials or capital goods, contemporary economic doctrines suggested that free trade produced progress, and politicians expected short-term gains in international affairs. Subsequent shifts in the international economy and in coalitions of domestic interest groups brought the brief flirtation with free trade on the continent to an early end.

A second theme of interest is the effect which state-formation had on mercantile activity. In chapter six, "European Port Cities," Kindleberger tentatively suggests the possibility that "gains-from-trade" merchants, who buy and sell goods in overseas markets, maintain significant economic ties beyond their own nation-state's boundaries, but that their economic interaction within the state's territorial jurisdiction is effectively limited by the cost of non-mechanized overland transport. "Ports dealt to a considerable extent with one another, rather than with the hinterland" (p. 173). The extra-national scope of mercantile activity leads to complex patterns of conflict and cooperation with administrative elites. For instance, in a chapter entitled "The Formation of Financial Centers," Kindleberger points out that "for money payments, there can be no doubt of the efficiency of a central financial market as the apex of a national system. . ." (p. 71). Before the mid-nineteenth century, however, banking had been associated with commerce, and was located in port cities. With the continental nation-states' promotion of railroads and public works projects, new national financial centers emerged in Paris or Berlin, that is, in the capital cities. Kindleberger's evidence implies that the centripetal movement of financial institutions to the administrative capital was attenuated or delayed in fragmented or in federal polities.

A third significant topic is the author's assessment of the relationship of mercantile to industrial activity. In "Commercial Expansion and the Industrial Revolution," Kindleberger asks "whether a commercial revolution—that is, a discontinuously rapid increase in commerce—was necessary to, sufficient for, or merely contributed to the industrial revolution?" (p. 135). Kindleberger argues that German industrialization took place despite the absence of a prior commercial expansion, and that the commercial revolution Holland underwent in the sixteenth and seventeenth centuries failed to produce an industrial economy. In Britain, however, a commercial revolution did, in all probability, supply the necessary preconditions for the first industrial transformation. With the sequence of commercialization and then industrialization established for the British case, Kindleberger proceeds in his final substantive essay to a deft analysis of the British and German economic rivalry in the nineteenth century. He claims that at the outset of the century "the internationally oriented Hansa cities and Frankfurt ... had limited connections with the [German] interior" (p. 189). By the time industrialization occurred, it developed within the jurisdiction of the Prussian-dominated customs union known as the Zollverein. Kindleberger's analysis suggests that those concerned with political development should consider: what would have been the political consequences of a dramatic Hanseatic commercial spurt prior to the industrialization of the Rhineland and Ruhr.

As a self-proclaimed "literary economist," Kindleberger tests the applicability of economic models with clarity and verve (p. 223). The absence of quantification does not restrain him from making predictions. Political scientists will undoubtedly find Kindleberger's analyses and projections to be enlightening and thought-provoking. *Economic Response* deserves a wide audience among specialists in comparative politics and international relations.

LEONARD HOCHBERG

Cornell University

Australia, New Zealand, and the Pacific Islands since the First World War. Edited by William S. Livingston and William Roger Louis. (Austin: University of Texas Press, 1979. Pp. x + 249. $17.95.)

This book is a collection of 12 essays which deal with developments in the last 60 years in the nations of the Southwest Pacific. The book is an outgrowth of the research activities associated with the University of Texas Collection of Southwest Pacificana, the largest collection of materials about that region found in America. Eleven of the contributors are currently associated with major universities in Australia and New Zealand, and they bring to their writings the fruits of their special knowledge and interest in the Southwest Pacific.

The 60-year period under survey has been one of profound change. In 1920, the area was regarded as little more than a British enclave, though located many thousands of miles from the mother country. After 1920, and especially after the start of the Second World War, three interacting forces have transformed the region. These forces have been the steady decline of British power, the rising influence of the United States, and the rapid increase of various forms of nationalism. The effects of these three interacting forces provide a continuing theme that runs through most of the contributions to this collection.

Three of the essays deal with books and other recent writings within the region. Of these, two are surveys of historical literature—one of Australia and the second of New Zealand—and both emphasize the sense of evolving national identity. There is also an essay about contemporary literature, which places particular emphasis on the evolution of literary consciousness, an important part of the evolving national identities of the region's people.

Three contributors are concerned with the international relations of Australia and New Zealand. During and since the Second World War, relations between the two countries have increased, but the problems of the "regrettable sense of superiority" of Australians in their dealings with New Zealand has not changed. The separate essays on the foreign policies of these two countries emphasize the relative decline of British influence, and the commensurate increase in the importance of the role played by the United States.

Two chapters are concerned with the indigenous peoples of Australia and New Zealand. Recently, Australian government policy on the Aborigines has changed from the earlier one of simple protection to a more positive policy of welfare and full citizenship rights. The Maori in New Zealand have long had a recognized position in their country's political structure, but the rapid move of the Maoris into urban centers has created unsolved economic and social problems.

Remaining essays in the collection include one on state-national relations in the Australian federal system, and one on the smaller recently independent nations in the Southwestern Pacific. In the concluding contribution, Hartley Grattan effectively synthesizes the ideas presented in the 11 earlier studies.

Faults are difficult to find in this excellent collection. One might have wished for greater coverage of the internal politics of the countries under consideration. The typical indiscriminate prejudice of academia in the Antipodes to favor their labor parties and to oppose the policies of more conservative parties shows through in several of the essays. In summation, this book makes a very useful contribution in filling the near-vacuum in recent American publications about Australia, New Zealand and the Southwest Pacific islands.

CHARLES KUNSMAN, JR.

San Jose State University

Political Values and the Educated Class in Africa. By Ali A. Mazrui. (Berkeley: University of California Press, 1978. Pp. xiv + 392. $18.50.)

This book of essays presents Ali Mazrui at his perplexing best: intellectually playful and deeply concerned; wide-ranging and intensely personal. The personal is essential to the book, and to non-initiates, it should be pointed out that Mazrui is one of the most effective intellectual links between Africa and the West. This role he has played with considerable éclat, in his voluminous writings, as president of the (American) African Studies Association, as the 1979 Reith Lecturer on the BBC, and perhaps most importantly, as head of the political science department at Uganda's Makerere University until forced to leave by Idi Amin. (This book was dedicated, before Amin's overthrow: "To Makerere! You, who were built for the future, must now await the future to rebuild you.")

As, in his own description, a "westernized or semi-westernized African intellectual" (p. 19), Mazrui has sought to articulate African ideas to Western audiences in terms which even they can understand. By rigorously eschewing the impassioned anti-Western diatribe that dulls thought and convinces only the already converted, Mazrui proceeds to draw unwary Westerners into the argument and then to prick their consciousness as a familiar idea, a cherished value is revealed in a new and disturbing light, the light in which an African might view it. For example, in the essay on "Christianity and Humanitarianism in African Diplomacy" the reader is treated not to denunciation of the Red Cross as an agent of imperialism, but to an opportunity to understand why African governments may perceive it as a dangerous political actor. As Mazrui concludes, "The very principle of humanitarianism has yet to recover from the full consequences of the Euro-Christian order while it lasted in Africa" (p. 163).

No less important, Mazrui stands as a spokesman for certain Western ideas in Africa, those ideas which he thinks can best be combined with Africa's own traditions of thought to produce true cultural liberation, both from the colonial past and from the inevitable reactions to that past led by non-intellectuals like those Mazrui styles the "lumpen-militariat." The ideas he espouses, and predicts will come into their own in the 1990s, could not now be more unfashionable: "The liberal values of tolerance, pluralism and individualism" (p. 373), rather than the equally Western, and vastly more fashionable ones of scientific socialism and centralization. Mazrui's use of Western ideas for African purposes begins always by decoupling those ideas from the West's possessive embrace and proceeds to transform those ideas into something wholly African. This process is most clearly evident in his inaugural lecture on taking the chair of political science at Makerere in 1966. It bears the title "Ancient Greece in African Thought," whose happy presumption is all the greater for the time and occasion at which it was delivered. Proceeding from the incontrovertible point that classical Greece existed before the idea of Europe had been invented, Mazrui weaves a many-stranded argument giving Africans as clear a claim on the Greek philosophical and political heritage as that of the barbarians to the north and northwest. "Just as it would be a mistake to let Europe nationalize Christianity, it would be a mistake to let her confiscate the Hellenic inheritance. The Greeks must be allowed to emerge as what they really are—the fathers not of a European civilization but of a universal modernity" (p. 97).

The centerpiece of the collection is the essay on "The African University as a Multinational Corporation." It proceeds from the premise that "both African universities and multinational business in the continent face a fundamental problem—*how to decolonize the process of modernization without ending it*" (p. 285), to investigate the multiple relations between economic and cultural dependency and between economic and cultural development. The university, he argues, has helped promote political independence, but at the cost of accentuating other dependent relationships. The solutions Mazrui advocates begin with the "domestication of modernity, a bid to relate it more firmly to local cultural and economic needs." Next, should come "diversification" in which "the foreign reference-group . . . becomes not only the West but also other non-African civilizations." The third stage involves "an attempt by the African continent as a whole to counter-penetrate western civilization itself" (p. 307). The parallel with strategies for economic independence is clear enough; so, too, is the parallel with Mazrui's own career.

Mazrui here, as in his other writings, presents his ideas through the form of the essay, a form which he manipulates with elegant control. Like the greatest masters of the form, he exploits the literary devices of paradox, reversal, and incongruous juxtaposition to startle the reader into discovering ideas while he weaves a complex narrative to bring the reader at the end into a state of participatory comprehension that few journal articles elicit. But the form has its costs. The demands of narrative movement, of balance, of aphoristic brilliance militate against the sustained presentation of evidence and argument. If reality does not fit form perfectly, there is a strong temptation to rearrange reality. Thus, in "The African University as a Multi-national Corporation," Mazrui argues that imperialism stems from two things, "a cluster of economic motives and a cluster of cultural imperatives" (p. 285). One suspects that political and strategic motives (imperatives?) are absent, not because the author thinks them nonexistent or subsumed in another category, but because he needs to highlight the economic and cultural categories so that he can develop the argument about the university in formal parallel. The points made by the use of the device are made brilliantly enough that one can well forgive the poetic license, unless of course one is there and then concerned about what does stimulate imperialism.

Mazrui at one point defines an intellectual as "a person who has the capacity to be fascinated by ideas, and has acquired the skill to handle some of those ideas effectively" (p. 437), and counterposes the intellectual to the scholar distinguished by his "meticulous care in handling the evidence" (p. 348). Should he be forced to choose, I suspect Mazrui would choose in line with the self-description quoted at the beginning of this review. And we are, on balance, the richer for it.

WILLIAM J. FOLTZ

Yale University

The Urban Origins of Rural Revolution: Elites and the Masses in Hunan Province, China, 1911–1927. By Angus W. McDonald, Jr. (Berkeley: University of California Press, 1979. Pp. xi + 369. $17.50.)

Angus McDonald's effort must initially be applauded. He has drawn extensively upon Chinese, Japanese and Western sources in an effort to focus on a tumultuous period of Chinese history. McDonald has not flinched in his attempt to overturn virtually every stone relevant to an analysis of the Hunanese political and social environment between 1911 and 1927. To any historian, the bibliography and the index are testimony to the author's comprehensiveness. Likewise, the book has appeal to anyone interested in Mao Ze-dong, since Hunan was Mao's native province. Finally, McDonald's selection of Hunan, as a province to be examined in depth, makes good methodological sense. While not a "bellwether district," Hunan was and is a balanced blend of urban and rural characteristics; this blend is an obvious advantage in an examination of the roots of a revolution that had both urban and rural dimensions.

McDonald immediately, and quite correctly, informs the reader that the tumult of the period involved "massive political, economic, and intellectual changes" (p. 1). This observation is not surprising since all revolutionary societies experience these transformations. What is worrisome, however, is the author's admission, "This book is also in several ways an experiment in history writing . . . it is shamelessly eclectic" (p. 1). While one might encourage experiments in "history writing," a "shamelessly eclectic" approach to the analysis of what is clearly an extraordinarily complex series of events is risky at best. This risk is not worth taking, particularly given the real need for more thorough analysis of this early period of the People's Republic's history.

Despite McDonald's thoroughness with regard to names, dates and events, at times the reader will have difficulty pulling together the theoretical themes the author suggests. For example, throughout the text McDonald implies there may well be three revolutions occurring simultaneously: (1) A revolt against the warlords, (2) A revolt against foreign imperialists, and (3) A "classic revolt involving class conflict. Although, admittedly a difficult task, the distinctions between, and the implications of, these different revolts urge greater refinement. In addition, a potentially interesting theoretical distinction between Mao's approach to organizing peasants and his approach to organizing urban labor is proffered, yet is not developed. Finally, three widely separated, and very brief, references are made to Riker's *The Theory of Political Coalitions* (Yale, 1963) in the context of the coalition politics that characterized the relationships between the young Chinese Communist party, the "left" Nationalists, and the "right" Nationalists. This intriguing topic is not at all developed.

In sum, McDonald's effort lacks some of the theoretical rigor to which political scientists have become accustomed. On the bright side, political scientists should be grateful for the attention to detail and the comprehensiveness which characterizes this book, with the idea in mind that much can be made of this raw material.

JOHN R. HANDELMAN

Washington & Lee University

Southeast Asian Transitions; Approaches Through Social History. Edited by Ruth T. McVey with Adrienne Suddard. (New Haven, Conn.: Yale University Press, 1978. Pp. ix + 242. $17.50.)

The editor of this volume places its five studies of the impact of colonialism on Southeast Asian local communities on the micro-historical level. The book, dedicated to the memory of Harry J. Benda, is the work of Benda's students and close associates. The collection of case studies can be considered a product of Benda's concern for the need of micro-historical investigations which could link "the specifics of local change to a larger concern for social pattern" (p. 5). The units for study are defined by the criteria of scale—"small and relatively simple"—and subordination—the relationship to the center. As for the first criterion, the communities range from the Trengganu Sultanate (Heather Sutherland) to the Cagayan Valley of northeast Luzon (Edilberto C. De Jesus) along with the Madiun Residency of East Java (Onghokham), Luzon's Pampanga province (John Larkin), and the diverse ethnic communities of Burma's Irrawaddy delta (Dorothy Hess Guyot). The second criterion is of far more importance in unifying the diverse materials intellectually. The analytical thread is the shift of power from local to central political units as colonial forces economically and bureaucratically subverted the social structures and value basis underpinning the authority of members of the traditional indigenous supra-village elite.

Sutherland traces the ultimately futile resistance of the Trengganu Sultanate to the challenge of the British presence ending in the adaptation of the elite to the administrative patterns of a bureaucratic state in which the norms of the Malayan Muslim state no longer provided the basis for the legitimacy in a society challenged by Malayan and Chinese immigration and external commercial interests. De Jesus analyzes the effect of the tobacco monopoly on the traditional elite (*principales*) of the Cagayan valley, 1786–1881. Policies of the monopoly resulted in social conservatism, with the result that the position of tradition ruling class was strengthened. De Jesus carefully points out, however, that a host of other factors tended to distinguish Cagayan from the rest of the Luzon provinces and that the impact of the tobacco monopoly was different. This underlines a central issue for micro-history: how to reach the macro-level. In the book at hand, can we go from discrete case studies to general propositions about social history and political change in Southeast Asia? The Onghokham contribution on the so-called Brotodiningrat affair illustrates the problem. Although cast within the framework of the general tension and conflict in the relationship between the Dutch colonial bureaucracy and the indigenous regents (*bupati*) in a system of "indirect" rule, the dynamic of this particular case stemmed from idiosyncratic psychological variables peculiar to the two protagonists: the bupati and the resident. This is not to deny the interest or validity of the conclusions in the wider discussion of the weakening of the power of the regents, but suggests that these are drawn a priori.

Dorothy Guyot's discussion of communal conflict in the Burmese delta starts from a different vantage point. Her concern is not the pattern of penetration of British rule, but the consequences of the disintegration of British authority in the region by the end of February 1942. Before Japanese control had been imposed in June, a communal war raged between the Burman and Karen communities. In part this was a function of the disruption of traditional society with its social roles, and the emerging salience of ethnicity as the two indigenous communities maneuvered for economic advantage. Focusing on one district in the heart of the delta, Guyot's micro-history is an excellent example of the importance of this kind of research since it does elucidate the terms of the ongoing ethnic conflict in Burma. More restricted in scope is John Larkin's essay on the Zarzuela theater in the Capampangan dialect through which, in the earliest years of the American occupation of the Philippines, the aspirations of the Pampangan economic elite found their outlet in musical comedy terms. Only Larkin, and then perhaps because of the nature of his subject, has been able to meet his editor's injunction to escape Western archives. Despite the deliberate effort to adopt a Southeast Asian stance, the authors continue to rely on colonial records and Western commentators.

McVey's introductory essay carries the argument beyond the impact of colonial power on the supra-village elite to its effect on peasant society. The general trend towards peasant impoverishment and dependency was ideologically accentuated by the increasing gap between peasantry and the elites, both colonial and indigenous. In their inability to bridge that gap, the modern national state, with its emphasis on bureaucratic control backed by coercive force, resembles the colonial state. In a proposition to which many caveats could be entered, McVey suggests that the Communist system in its ability to engage the "common people" through revolutionary revitalization is, "in important ways more 'natural' to the precolonial traditions of most major Southeast Asian polities" (p. 30). But, she adds, the Marxist-Leninist regimes also show a tendency to bureaucratic control from above, and in that respect may prove a more effective instrument of domination.

DONALD E. WEATHERBEE

University of South Carolina

Comparative Legislatures. By Michael L. Mezey. (Durham, N.C.: Duke University Press, 1979. Pp. xiv + 317. $17.75.)

The study of comparative legislative politics is still at an early stage of development. Michael Mezey attempts to take the field a step forward. He constructs a concise, fivefold classification of contemporary legislatures. He then applies his classificatory scheme to determine how much it helps organize existing evidence on legislative structure and behavior as these relate to policy making, representation, and system maintenance. The book is laudable in its breadth and in its effort to indicate the specific legislative arena in which a particular kind of activity will occur for each class of legislature.

Yet while there are valuable insights to be found in this study, on the whole the book is unsuccessful. For a classificatory scheme to be useful, the classes must not be so narrow as to exclude pertinent evidence, there needs to be a consistency between how a class is defined and the legislatures the author places in that class, and the overall scheme should help to organize phenomena that lie outside the scheme itself. Mezey's system of classification does not successfully deal with any of these objectives.

For purposes of brevity, this review focuses on Mezey's treatment of one of the central topics of the book, the policy-making role of legislatures. Keeping with the tradition of most classificatory schemes on legislatures, Mezey classifies legislatures partly according to how much relative policy-making power they have compared to the executive (p. 23). His method of categorizing policy-making power, however, considers only the degree to which a legislature modifies or rejects *executive* proposals (p. 26). Such an approach largely ignores the possibility that legislatures differ substantially in the extent to which they initiate important legislation on their own. An inadequate classification results. For example, Mezey places both the Colombian Congress and the British House of Commons in the same class: they are legislatures with a modest policy-making power in that they sometimes modify but hardly ever reject executive initiatives. At the same time, Mezey points out almost parenthetically, that about 60 percent of the "more important" laws adopted in Colombia are initiated by the Congress and *not* the executive (p. 114). The British House of Commons can hardly be said to initiate even 10 percent of the more important laws adopted in the United Kingdom. Surely there is a problem here in classifying the two legislatures as the same type with respect to policy-making power, even if we accept Mezey's assertion that both treat executive initiatives in broadly the same fashion. Classification of quite a few of the legislatures (among others, the Costa Rican Congress and the West German Bundestag) are similarly affected by definitional considerations such as these.

Beyond problems arising from Mezey's method of categorization, Mezey is inconsistent even in his own application of the classifications. That is, even if one accepts Mezey's definitions, often he simply misclassifies legislatures by neglecting to follow the definitions with which he began. Taking another legislature as an example, the Polish Seym, Mezey classifies the Seym as a "minimalist" legislature, or one "that can neither modify nor reject proposals" (p. 27). But when Mezey later goes on to describe the Seym, he writes: "From 1957–1969 the Seym passed 327 statutes of which only sixty were approved without modification" (p. 136). Further, some of the Seym's amendments "were so extensive as to be almost a substitution of the original measure" (p. 136). Nowhere does Mezey tell us why, in light of this kind of evidence, he places the Seym as a "minimalist" legislature that can neither modify nor reject proposals.

Quite apart from these problems, another area of concern is whether or not the classifications help coherently to organize phenomena that lie outside the classificatory scheme. If we know, for example, that a legislature fits the classification Mezey calls an "active" legislature (an "active" legislature has strong policy-making power and is considered legitimate by the elite and the general public), what else can one then say about that legislature? Mezey classifies both the United States Congress and the Costa Rican Congress as "active" legislatures. Yet, despite the similar classification, it is virtually a random proposition as to whether the two legislatures are alike in any other way. Sometimes they are, but at least as often they are not. And still other times one cannot say because Mezey cites evidence about one of the legislatures while completely ignoring the other. This varied assortment of results occurs with regard to each of Mezey's classes of legislature, the consequence of which is to produce a certain sense of confusion. Although some of this confusion might have been reduced had Mezey defined and applied his classificatory scheme more properly at the start, as it stands now there are few areas in which the classificatory scheme ends by making comparative legislative organization and behavior discernibly more coherent than before.

Finally, there is a serious weakness in the scope of the evidence the author employs. As befits a study of such coverage, Mezey relies for his evidence almost entirely on others' research. Yet, for some reason, he entirely ignores non-English sources. Not one foreign-language source is cited in this entire study of legislatures across the world. The result is that there are needless gaps in evidence that foreign language sources could have filled. In addition, the author at times reaches conclusions which research reported in non-English sources would dispute. To illustrate again with an example from the area of policy making, Mezey cites English language sources to reach the conclusion that members of the German Bundestag are not much engaged in the policy-formulation process (p. 87). In so doing, he ignores a series of sources in German, including the detailed work of Domes, which provide rather abundant evidence indicating an opposite conclusion.

JOHN E. SCHWARZ

University of Arizona

Queen's Rebels: Ulster Loyalism in Historical Perspective. By David W. Miller. (New York: Barnes and Noble, 1979. Pp. xiii + 194. $22.00.)

David Miller's *Queen's Rebels* is an excellent complement to his *Church, State and Nation in Ireland, 1898–1921* (Macmillan, 1973) which studied the Catholic nation. Along with Peter Gibbon's *Origins of Ulster Unionism* (Manchester, 1975), it is the best work I have read on Ulster Protestant political history. As a single source Miller is superior to Gibbon in his fuller treatment of the present century—Gibbon's most valuable insights come from periods of early industrialization, the founding of the Orange Order in the 1790s particularly, but Gibbon does have forthcoming *The State in Northern Ireland, 1921–72,* with P. Bew and H. Patterson. Miller is also to be commended for his fine standard English. Gibbon's message is sometimes obscured by his use of Anglo-Althusserian "sociologese."

Both Miller books focus on the concept of nation, developing nationalism, and their relation to state authority within the two Irish "communities." *Queen's Rebels* is premised on Miller's oft-repeated theme that the reason Ulster Protestants are so difficult for the rest of the world to comprehend—and also why their concept of nationality is ambiguous even to themselves—is that their traditional political values, especially in relation to sovereign authority, are pre-modern, specifically, that they have very little concept of nationality in the modern sense. The Irish Catholic community became a nation. Their definition of their nationality, however, is at best ambiguous about including the Protestants within it. Miller demonstrates that the Ulster Protestants have been a clearly distinguishable political community but have neither become a nation unto themselves (I agree) nor fully integrated into British nationality. (But, here, with the concept of British nationality, we come to a problem difficult to categorize in comparative terms. Advocates of Scottish and Welsh nationalism, like Tom Nairn, argue that there never was any British nation.)

This book provides much useful detail and thoughtful analysis as to how the Protestants got this way. But I have two criticisms relating to Miller's insistence on the peculiarity of Ulster Protestant abnormality regarding national consciousness. The first criticism is his overemphasis on the trans-Rhenish, organic-romantic understanding of nation which has, admittedly, dominated the world—but not its English-speaking parts *except* for Catholic Ireland—since the French Revolution. (Miller acknowledges Anthony D. Smith but ignores his chronology of nationalisms.) The second criticism stems from the ill-defined character of British nationality which has contributed to a vacillating policy toward Ulster Protestants on the part of British governments. Now, this lack of clarity in British national self-perception is no fault of Miller's, but if he had better placed the Ulster conception in its necessary pan-British context, Ulster Protestants would seem less peculiar.

Miller's tracing of the Ulster "peculiarity" to the "banding" and "covenanting" traditions of early modern Scotland, as well as early Whig doctrines of conditional monarchy in combination with unstable frontier conditions, etc., provides rich insights of continuing relevance. It is just that there is a more direct explanation which is continually neglected: the ambiguity and/or division in British ruling circles for 100 years or more as to whether the Ulster "Loyalists" were, or should be, British. This ambiguity—and sometime hostility—forced them to behave as the national deviants that they are. (See pp. 117 and 162 for evidence of Miller's skipping over the implications of London's "ambiguity." He recognizes it, if just barely, in footnote 8, p. 167.)

Miller asks: Is "[(1)] Protestant Ulster a *distinct nation* like Scotland and Wales within a *multi-national* community, the United Kingdom? Or . . . is . . . the human community embraced by the United Kingdom . . . a 'nation,' of which Ulster Protestants consider themselves members?" He answers, "Actually neither of these propositions is quite satisfactory. The Ulster Protestant community is like Scotland (before the recent rise of national separatism. . .) in that it has exhibited no clear determination to be politically separated from the United Kingdom. [(2)] On the other hand, it is unlike Scotland in that whereas Scottish national feeling has *until recently* been fully compatible with a sense of 'British' nationality legitimising the Westminster regime, the Ulster Protestant community has evoked a kind of group loyalty incompatible with acceptance of the full implications of British nationality" (p. 46, emphasis added).

My comments on the above are as follows: (1) Scotland and Wales—for the great majority of their peoples—are also not distinct nations. The nation for them is also the United Kingdom—or at least, now, mainland Britain. (On pp. 64–65 Miller concurs with me.) Richard Rose has popularized the notion of the UK as a "multi-national" state. I think, to date, that is a misapplication of the term. The UK has the potential of becoming such; so too does the U.S. Britain has a "multi-faceted" but "asymmetrical" (i.e., English-dominated) conception of nationality very different from the nineteenth-century European norm. There are clear historical reasons for this deviation, but it still falls this side of modern nationhood in a subdivision that also embraces United States national consciousness. (Miller recommends David Potter, *The South and the Sectional Conflict*—in which I discovered a brilliant essay on comparative nationalism—but he should have made more use of it.) If one wants to study anational consciousness in advanced society, Anglo-Canada is even a better field than Protestant Ulster.

(2) For Miller "the full implications of British nationality" could be reworded as sufficient trust in the national political system to accept its decisions or to limit opposition to constitutional means (see esp. p. 103). This definition would rule out Ulster Protestants in several instances. But the utility of this definition is weakened by the mass actions of rank and file *English* labor in 1972 and 1974 when a Tory government was in power. The main reason that Ulster Protestants are deviant is that since 1886 they have had reason to question whether several British governments considered them co-nationals.

A shortcoming in *Queen's Rebels*, as contrasted with Gibbon's *Origins*, is Miller's lack of attention to divergent Protestant class and sectoral interests and how they were reconciled under the UK and by the Unionist party when it had its own autonomous provincial government, Stormont, from 1921–72. One of Gibbon's most imaginative chapters analyzes the contrasting riot behavior of Belfast Protestants on the Shankill (led by shipbuilders) and in Sandy Row (more laborers and lesser skilled workers). Although Gibbon makes another point, he provides evidence that workers in export industries where there was British-wide labor migration and British unions (e.g., shipbuilding) thought in pan-British political terms by comparison with those in local and Ulster-concentrated industries (which, like fine linens, although export-oriented was traditional to the province).

Miller, pp. 82–86, on the changing ecclesiology and *Weltanschauung* of Ulster Presbyterianism during early industrialization, should be read by anyone interested in the relationship between "modernization" and changing values.

For those tempted to see the future solution to the Ulster problem in "normal" nationalist/separatist terms, the epilogue on the post-1972 London direct-rule period could be a valuable corrective. Miller characterizes the attitude of one quasi-nationalist Protestant faction in words that would suit an independent Ulster state if such is to be. "So this is a reluctant matter-of-fact nationalism, perhaps a nationalism of despair, a nationalism arising out of immediate events and circumstances, not out of long-term social processes" (p. 154). We have seen a nationalism of short-term "social processes" arise in Eritrea, become a mass force, and perhaps be destroyed, all within the span of a generation. Miller does hold that Ulster Protestants should be regarded as a legitimate political majority entitled to remain politically separate from the Irish Republic even though they do not fully meet any definition of nationhood.

ROBERT ST.-CYR

Greenlawn, New York

Politics and Government in Malaysia. By R. S. Milne and Diane K. Mauzy. (Vancouver: University of British Columbia Press, 1978. Pp. ix + 406. $15.00.)

For political scientists Malaysia holds unusual challenges. Its global strategic position further enhanced by recent shifts in superpower involvements invites special consideration; its internal political development which may hold lessons for others deserves detailed scholarly examination.

Like many other newly independent states, the country is marked by social cleavages, economic disparities, and normative dissonances, but unlike most of them it has sustained economic growth and maintained a democratic system with six national elections and the orderly succession of three prime ministers. R. S. Milne has closely followed the Malaysian development process. Through a steady flow of publications (at times in collaboration with K. J. Ratnam), he has provided much of the hard data and the analytical insights now available. His latest book, with the able support of Diane K. Mauzy, continues this tradition and his high scholarly standards.

Politics and Government in Malaysia will serve well as a textbook. It treats comprehensively the basic subjects of historical evolution, economic policy and development, constitutional provisions, formal and informal structures, political processes, and foreign policy. The organization is clear; the style is articulate; the data and analysis are up-to-date. It has one other virtue: it is compact enough to be useful in introductory classes.

Politics and Government in Malaysia, however, is more than a textbook. Heavily footnoted, it effectively integrates all the significant research on the subject. Most interesting is the discussion of "political parties and interest groups," which forms the central portion of the book. In 1969 there was perhaps a chance that through the *Gerakan* a national (multi-communal) opposition party would emerge, leading ultimately to a viable, two-party system. Milne and Mauzy, mostly through their own new research, detail the elements which aborted this process: the failure of the *Gerakan*, the establishment of the National Front, and a consequent polarization between the constitutional coalition and the overtly (and/or covertly) anti-constitutional opposition.

Indeed, the book could have qualified as a study of political development had it addressed more directly and more extensively the issue of the relevance of Western democratic models. Conventional wisdom takes it for granted that (1) democratic systems can be maintained in any existing country and (2) while some variance in form and procedures can and do occur, the vital core of such systems is epitomized by formulas successfully developed in the West. Still, it may well be that the social, economic, and political conditions, quite apart from the time framework available for development in the newly independent (post-colonial) states of Asia and Africa, are fundamentally different from those which provided a peculiarly fertile environment for a very special variety of democratic systems in the West. The difference is essentially irreconcilable. In the former case, Western models are applicable: the task may be to follow set precedents toward an already demonstrated goal with progress a function of time and political management. If, however, the latter position more accurately represents reality, then any effort to transplant Western models or to apply Western standards of democracy is not only useless but also will lead predictably to disorder and violence. No uniform notion of a democratic system, no standardized blueprint toward its attainment can serve its purpose. Nothing less than new, innovative designs will do. They will have to be produced by political entrepreneurship through practical and very risky experimentation—admittedly quite a challenge, but really no greater than the one faced and mastered by the founding fathers of the United States.

The utility of Western models for Malaysian political development largely depends on the depth and intractability of its communal discontinuities. If social and cultural heterogeneity is bound together by a national focus of orientation or at least by a consensus on the legitimacy of political rules and institutions, they may serve as a guide and standard. If, on the other hand, the country is *dominated* by communal cleavages, then at least for the time being they are irrelevant.

The authors repeatedly and unequivocally take the position of radical communal discontinuities in Malaysia, and yet throughout their analysis their criteria are generally Western-oriented: occasionally the pluralist formula, more often the recently fashionable "consociational" model. This kind of ambivalence becomes explicit in the final chapter. It makes a point of the persistence of the (Malay) personal style of rule and once again restates the dominance of communalism. Against this background, however, follows the argument (p. 364): (1) a statement that "it is . . . desirable that . . . people should feel loyal to their country"; (2) a quick redefinition of this loyalty as "nationhood"; (3) the assertion that "the task of nation-building must be deliberately undertaken"; (4) that this task must involve the breaking down of communal barriers. In short, loyalty to Malaysia can be developed only by the elimination of communalism. To be sure, this is the orthodox Western position—but is it necessarily the only, and for Malaysia the most feasible, position?

Turning to strategy options for nation-building, the authors limit these "analytically" to four: assimilation, leaving things as they are, partition, and chaos—surely an all-too-arbitrary and unimaginative set. Then follows this sequence of ideas suggesting "an answer" for the near future: (1) for legitimacy, short-run effectiveness may be substituted; (2) effectiveness requires economic growth; and (3) economic growth depends on "social mobilization" or "modernization" (pp. 379–80). Insightful as these ideas are regarding Western democratic development, when raised to general propositions, their merit is rather dubious. In the end, the authors themselves swing back and emphasize how politically perilous this "answer" would be in Malaysia and search for evidence that it could be made to work. The conclusion suggests that they are not much satisfied with the results.

Milne and Mauzy will undoubtedly continue their valuable contributions to the study of Malaysian government and politics. If they agree with this review, they can be counted on to remove the one major limitation of this excellent book.

KARL VON VORYS

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Política, Vivienda Popular y el Proceso de Toma de Decisiones en Colombia: Análisis de la Coyuntura Actual y Viabilidad de las Soluciones Propuestas Durante el Frente Nacional. By Gabriel Murillo C. and Elisabeth Ungar B. (Bogotá: Editora Guadalupe Ltda., 1978. Pp. 367. Price not given.)

Colombia, along with many other low-income countries, has a serious housing deficit: recent estimates are that more than one-fourth of the population is improperly housed.

This multi-authored volume, published to commemorate the thirtieth anniversary of the founding of the Department of Political Science at the Universidad de los Andes, brings together a great quantity of material relating to low-cost public housing in Colombia; indeed, nearly every aspect of the problem is discussed at some point by authors Murillo and Ungar.

The following subject areas are among those dealt with in those portions of the book that review and synthesize secondary data: reasons for the existence of the problem; recommendations and solutions that have been proposed; quantitative and qualitative analyses of the problem; and the evaluations of housing policy from the perspective of various agencies that deal with the problem of low cost housing.

The most insightful and solid chapter of the book is the ninth. Although curiously it appears to be more an interpolation than an integral part of the work, it contains original data that are central to the investigation of several significant questions related to the present reality of low-cost housing in Colombia. Nearly 500 heads of households in six housing projects were interviewed.

Among other things, the data reveal that many families in the projects are not among the lowest income categories. The author attributes this in part to regulations that require steady sources of income from applicants so that the housing agencies can amortize the costs of the projects.

Because chapter 9 depends entirely on survey data, the accuracy of the responses to many of the interview items is open to question; indeed, it would seem that official government and census data could have been used productively. In spite of this and other methodological difficulties, the research project described in this portion of the volume, as well as several previous projects, clearly demonstrate that the Department at Los Andes is able to launch sound data-gathering and analytical efforts.

Appropriately Murillo and Ungar do not avoid a discussion of the basic political problems associated with what they rightly perceive to be an intolerable scenario in which millions of people are clearly disadvantaged. They recognize that a capitalist system tends to produce a non-egalitarian distribution of wealth, but they criticize those who act under the pretext that no solutions are possible under capitalism. They maintain that much more remains to be discovered about the complex housing problem and the decision-making process in Colombia (in fact, the surface barely is scratched in this study), and that from their point of view "those students who thoroughly familiarize themselves with both the theory and the reality of the housing problem ... moving from the isolation of the scientific context to the world of direct participation" can exercise (and have exercised) influence over policy.

But this optimistic note is accompanied by a sense of urgency. The likelihood is that if the present system is to remain intact, changes must occur. The solutions to the problems of great numbers of Colombians, problems not only of housing but of other basic human needs, must involve a collaborative effort of government, the private sector and concerned individuals with intellectual resources and a moral commitment.

REID READING

University of Pittsburgh

Towards Socialism in Tanzania. Edited by Bismarck U. Mwansasu and Cranford Pratt. (Buffalo, N.J.: University of Toronto Press, 1979. Pp. x + 243. $20.00.)

Tanzania has been a popular subject of study for several reasons, not the least of which has been the heated debate over the nature of its transition to socialism. The book under review—a collection of essays stemming from a 1976 Toronto conference—is an important contribution to that debate. The book includes an introduction and eight essays which are representative of left (generally critical) and right (generally pro-regime) socialist thinking on Tanzania, and the diverse opinion makes for good reading. The editors are partisan members of the socialist right, and Cranford Pratt has engaged in a wide-ranging debate on Tanzania with John Saul in recent issues of the *Canadian Journal of African Studies.*

The editors provide an introduction, followed by Reginald Green who presents considerable detail for an argument that important strides have been made toward the regime's economic goals. Ian Parker uses the case of the National Development Corporation to illustrate nicely how societal contradictions pervade the parastatal sector. John Loxley traces the evolution of monetary institution policy within a state-society framework outlined elsewhere by John Saul. In a gem of an essay, Jonathan Barker criticizes left and right analyses of the regime's failure to put over its rural communal production policy. He finds that the right should look more closely at the operation of communal incentives before assuming that small-holder production is superior, while the left should look beyond faulty implementation explanations to problems of internal village organization. Jannik Boesen then uses the faulty implementation idea to explain the communal village experience in West Lake Region. Adolpho Mascarenhas lays out some issues to be addressed in the aftermath of villagization, and Bismarck Mwansasu traces the legal evolution of the ruling party with an identity apart from the bureaucracy.

Perhaps the most important contribution is the final essay by Cranford Pratt, "Tanzania's Transition to Socialism: Reflections of a Democratic Socialist." Pratt argues that pessimistic left commentaries which doubt the reality of a transition tend to ignore substantial evidence to the contrary. He also asserts, wrongly, I think, that the left advocated a "vanguard" brand of party which would have undermined the democratic content of the transition. The left has consistently called for strengthening a peasant and worker presence in the party and in other institutions, and it has advocated a more cohesive and practical ideology of development; but with only a few exceptions and despite often vague prescriptions, there is no reason to assume a hidden agenda promoting, or necessarily resulting in, an exclusive elite-led party.

Pratt cites evidence that Tanzania has made progress towards socialist goals defined by President Nyerere: the achievement of more equality; an increase in democratic participation; the development of socialist values; and the achievement of greater national self-reliance. Pratt makes a plausible case for all but one, that of increased popular participation. Despite assertions that popular influence is at work, there is no significant evidence of it provided in this volume or in other recent published work. Pratt himself has doubts but argues that the leaders "have continued to try" (p. 218). Given recent political history, however, one wonders how hard they tried. Trade unions and cooperatives are tightly controlled, district councils have been abolished, and recent administrative decentralization has not involved substantial devolution of power beyond the bureaucracy. A worker control movement, itself triggered by the state, was put down rather ruthlessly instead of building on popular initiative. Socialist villages, also called for by the state, were not nurtured with due consideration for their necessary degree of autonomy as well as their need for useful external advice. The media is controlled by the state and Pratt acknowledges that significant organized opposition is not allowed. There are regular elections to the National Assembly, but that body is weak while the parameters of election discussion are severely circumscribed. Meanwhile elections within the party are only indirect. Pratt is well aware of these facts and thus he must rest his case on the promise rather than realization of participation, although the historical trend is best seen as retrenchment.

If retrenchment has occurred in Tanzania, as it has in most of Africa, then there is room for more anxiety about Tanzania's socialist future and there is a need for serious strategic thinking about how to expand peasant and worker leverage on the political system. Here the writing of the political left is particularly relevant. It would appear that without compromising socialist direction the party could be opened up, and older institutions like self-help groups, cooperatives, and trade unions might be revived as a means of affording reasonably autonomous institutional vehicles for the defense of peasant and worker interests in the short run, and the possibility of a more effective politics in the future.

The essays present useful empirical material not found elsewhere, but more importantly they, and particularly Pratt, help clarify theoretical positions on the transition to socialism in Tanzania. The volume should be consulted by anyone interested in recent Tanzanian experience as well as those interested in transition questions in general.

FRANK HOLMQUIST

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Waehlerverhalten in der Bundesrepublik Deutschland: Studien zu ausgewaehlten Problemen der Wahlforschung aus Anlass der Bundestagswahl 1976 [Voter Behavior in the Federal Republic of Germany: Studies on Selected Problems of Electoral Research in Connection with the 1976 Federal Elections]. Edited by Dieter Oberndoerfer. (Berlin: Duncker & Humblot, 1978. Pp. 531. DM 128.)

Both in the United States and in Germany, a good deal of research on voting behavior and elections, and certainly the bulk of data collection activity in this area, takes place in the trenches of electoral campaigns rather than in the halls of academia. This fact is easy to overlook since, at least in the U.S., much of that body of research and data has typically been ad hoc, has remained proprietary, and has resided outside the public domain; as a result, there is as yet little scholarly knowledge (or data) bearing on, say, the dynamics of candidate image structures, of campaign support mobilization, of governmental performance perceptions, to name but a few obvious lacunae.

In Germany, thanks to the greater strength and continuity of party organizations (especially between campaigns), the political parties have, for all practical purposes, acquired quasi-permanent research arms, which in turn have been able to build research staffs, develop data collection projects, and pursue analytic concerns with a more long-term perspective than their American counterparts. Whether actual data collection is handled by an in-house survey field organization (as in the case of the SPD-connected *infas* [Institute for Applied Social Research]) or is contracted out to commercial institutes (as in the case of the CDU-connected *SFK* [Social Research Institutes of the Konrad Adenauer Foundation]), these "private"/public party-supported research organizations have emerged side by side with the "traditional" academic centers of electoral sociology—Cologne (with *ZA* [Central Archive for Empirical Social Research]) and Mannheim (with ZUMA [Center for Surveys and Methods of Analysis])—as a mainstay of ongoing research and a professional base for junior scholars. In fact, this "non-academic" cadre of social scientists—readily identifiable by their inevitable pilgrimage to Michigan and increasingly visible by their publications in the German scholarly literature—has contributed significantly to the diffusion, interpretation, and adaptation of American research on voting behavior within the mainstream of German academic discourse. Moreover, at least in the case of *SFK* studies, the major data sets (which include three large multi-wave panels) have been/will be placed in the public domain (via *ZA*) for access and secondary analysis by the scientific community.

The present volume, compiled under the editorship and tenure as *SFK* director (from 1976 to 1978, after Werner Kaltefleiter) of Dieter Oberndoerfer, offers a collection of forays into fundamental areas of contemporary electoral research by members of the *SFK* research staff, based on data up to and including the 1976 federal elections. Between opening pieces on the relationship of empirical research to political practice (Oberndoerfer) and normative theory (Garding), and closing pieces on the relationship of voter preferences to economic concerns (Jung) and candidate perceptions (Eltermann), the main body of the volume consists of a series of studies focusing on both attitudinal and systemic voter-party linkage structures: ideological perceptions (Does), partisan evaluations (Roehrig/Stadie), social support (Noetzel), party identification (Gluchowski), and issue positions (Garding). The volume is neither a synopsis nor a selection of studies of the 1976 elections, but rather a sample of in-depth analytic efforts at dealing with current topical problems of electoral research reaching beyond a specific election, or a specific country (though the substantive context is, of course, Germany); as such it is comparable in spirit and depth, if perhaps not in breadth, with the 1977 issue of *PVS* (Politische Vierteljahresschrift) "Wahlsoziologie Heute" edited by Max Kaase.

A detailed review of individual pieces is clearly impossible here. Most noteworthy perhaps is Gluchowski's analysis of the party identification concept from a construct validation perspective—probably the most theoretically sophisticated and empirically compelling treatment to date of a long-standing U.S. export. This is complemented by Garding's analysis of dissonance reduction in the face of party vs. issue conflict, which starts from the opposite end (issue voting) and from a different focus (his introductory essay on normative theory, based largely on D. F. Thompson) yet provides corroborative evidence on the role of party loyalties and on the impact of parties qua political actors. Also interesting to note is the dominance of negative over positive party discriminations revealed by the analyses of both Does and Roehrig/Stadie—a potentially promising candidate for export in return. Intriguing in a different way, finally, is Noetzel's exercise in overkill which leaves the ill-fated "spiral of silence" hypothesis of public opinion processes hanged, drawn and quartered. Unfortunately the volume does not include a contribution from the group engaged in the study of political communication processes via computer content analysis at *SFK;* but then, that would go beyond "voter behavior."

The overall caliber of the contributions, with minor exceptions, easily stands on par with contemporary U.S. output and generally reveals a more acute and incisive (if occasionally a bit tedious or pretentious) critical-theoretical treatment than found with most American writers. This may not merely be a reflection of contrasting continental-European and Anglo-Saxon intellectual traditions; it may also be a self-conscious result of the fact that empirical social research has continuously been on the defensive in Germany—first against the momentum of legalistic/philosophical "traditions" of social thought, and then, without respite, against the onslaught of a new anti-empirical dogmatism beyond "bourgeois" social science. Indeed, the generation represented in the present volume may be the first cohort, not of a growing swell but of a continuing trickle, of modern-trained German social scientists.

As it stands, the volume benefits more from the conceptual/analytic sophistication of the various contributions than it suffers from their technical/methodological limitations. It should be of considerable interest to students of voting behavior and elections, not just to students of German politics. In fact, like the special issue of *PVS* (which has been published in English by Sage), this collection deserves to be made accessible to a larger audience, perhaps with some deliberate editorial compression, as part of a converging stream of multi-national theory-building and research—still the exception rather than the rule in the social sciences.

LUTZ ERBRING

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History of Vietnamese Communism, 1925–1976. By Douglas Pike. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. xiv + 181. $5.95, paper.)

Douglas Pike's study of North Vietnam's Communist party is one of a series of Hoover Institution histories of communist parties. The series' general editor, Richard F. Staar, has set forth the major aspects of communist parties to be covered in the series: history, leading personnel, membership characteristics, domestic impact, relations with other communist parties, strategies, and contemporary problems and prospects. Pike's volume is neither an analysis of Vietnamese political life nor an assessment of the dynamics of the Vietnamese Communist party itself. Instead, the volume is a descriptive account of party organization and personnel, mainly during the period 1930–1960.

Pike begins promisingly by setting forth four major themes that shaped the founders of Vietnamese communism: (1) A heritage of traditionalism, (2) a heritage of colonialism, (3) the spirit of *doc lap* (independence), and (4) the turbulent social scene and political malaise prior to the birth of Vietnamese communism. However, he does not follow through with these "four currents in the long, winding river of Vietnamese history"; instead, in great detail but without clear organization, he chronicles the names and chronology associated with Vietnamese communism. His staccato-like outline style has the advantage of brevity, but the disadvantage of turgid, unfelicitous prose which becomes tiresome.

The one disappointment is the lack of material devoted to the 1960–1975 period. Pike argues that the party history of that time was largely a record of events that did not happen (p. 127). There were no internal upheavals, no struggle for power following Ho Chi Minh's death, no pressures from the Soviet Union or China. Yet this is the very period when the Vietnamese Communist party consolidated its power over the National Liberation Front (a group which Pike does not discuss) in the South, and when North Vietnam carefully and brilliantly played mediator between Russia and China while receiving aid from both sides. In contrast to Pike's detailed description of the 1930–1960 period, his cursory overview of the 1960–1975 period is superficial. It is all the more disappointing since Pike is a recognized expert on the later period, while the earlier era has already been well documented in Robert Turner, *Vietnamese Communism: Its Origins and Development* (also published by the Hoover Institution).

Pike praises various aspects of the Communist movement in Vietnam. He refers to the organization's "breadth of vision," "sophisticated revolutionary organization" and "idealism of its followers." At the same time he notes the tight discipline and doctrinal purity which meant that serious mistakes were punished by death. He documents the use of violence and terror used by party officials up to the present. He concludes that the party, in the end, was a success, "for it created a well-disciplined, submissive society, ruled by the all-intrusive Party properly guided by communism—in fact, the very model of a Marxist-Leninist society" (p. 107).

In his concluding chapter Pike spells out possible future trends in Vietnam's foreign relations. He correctly foresaw the Vietnamese move to create an Indochina Federation, composed of Laos, Kampuchea, and Vietnam. And he hit target when he noted that relations with the PRC would likely be punctuated by hostile incidents. However, he underestimated the degree of dependence Vietnam would have on the Soviet Union and he suggested that the Vietnamese Communists "will [probably] not engage in what might be called old-fashioned aggression—the dispatch of tanks across international boundaries" (p. 149).

The comprehensive bibliography and detailed index enhances the usefulness of this book for scholars and students interested in the organizational structure of the North Vietnamese Communist party.

CLARK D. NEHER

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The Rational Peasant: The Political Economy of Rural Society in Vietnam. By Samuel L. Popkin. (Berkeley: University of California Press, 1979. Pp. xxi + 306. $14.95, cloth; $4.95, paper.)

Samuel Popkin proposes a model of peasant society based on what he calls "political economy" in opposition to a prevailing model he terms "moral economy." The first two chapters sketch and contrast these two paradigms of the rural world; the remainder of the book seeks to demonstrate the superiority of the author's model through the use of historical materials on peasant society and movements in colonial Vietnam.

The "moral economy" model is, the author admits, his own construct from various theorists, rooted in the tradition of Karl Polanyi. Among these, Eric Wolf and James C. Scott figure prominently; Scott's recent book, *The Moral Economy of the Peasant* (Yale, 1976), presumably supplies the title for Popkin's counterfoil. Perhaps inevitably, much of the nuance of these theorists is missing from the author's construct; at times Popkin seems to saddle his absentee protagonists with an indefensible portrait of pre-capitalist village society which is an amalgam of Durkheim, Rousseau, and the Good Samaritan—noble, altruistic, *gemeinschaft.* In the moral economy view, economic institutions and norms are "embedded in" (to use Polanyi's phrase) broader social norms and institutions. There can be a "just price" in such communities, for example, whereas the notion makes no sense in a market economy (a continuing problem for formulations such as "excess profits"). The tensions, strains, and dislocations generated by the interaction of such village societies with world market forces—particularly the commercialization of agriculture and penetration of capitalist relations and institutions—produce inexplicable and unacceptable outcomes from the village perspective and thus weaken traditional elite legitimacy and provide the impetus for peasant insurrection. Defined in opposition to the moral economy Paradigm is Popkin's model: peasant society as an aggregation of the rational individualistic maximizers of classical economics, drawing heavily on theorists such as Mancur Olson, Kenneth Arrow, and Norman Frohlich, self-consciously within the "rational choice" tradition. There is neither moral economy nor community.

Popkin in effect promises *Homo economicus rusticus,* whose behavior is governed in both economic and non-economic spheres by strict canons of economic rationality. The critical question, the fulcrum of the book, is: what does rationality mean? A strict and narrow definition produces powerful theory but the familiar difficulty of capturing the rich complexity of human behavior; a broader definition sacrifices power and parsimony.

The author's definition of rationality (p. 31) argues that "individuals evaluate the possible outcomes associated with their choices in accordance with their preferences and values." But then the criterion of rationality is reduced to the idiosyncratic and infinitely variable range of individual "preferences and values." This conceptual problem plagues the analysis throughout. For example, we find seemingly strong nomothetic statements: "Whether a self-interested peasant will or will not contribute to a collective action depends on individual—not group—benefits" (p. 251). If so, it seems paradoxical that the Viet Minh Popkin is analyzing ever emerged, since the average individual expectation in protracted peasant insurrection must be protracted hardship or death. But the author acknowledges the obvious: peasants made some contributions "which were not stimulated by any expectation of future selective payoff." Moreover, the Viet Minh mobilization "emphasizes how important internalized feelings of duty or ethic can be" (p. 223). Again (p. 254), of the reasons why peasants contribute to collective action, the first is "for reason of ethic, conscience, or altruism." Precisely, but then what of the powerful law of self-interested amoral "rational" behavior?

Popkin derives important propositions from his model, but his case study merely illustrates their plausibility, never their accuracy or universality. Bold assertions about risks, investment strategies and payoffs are presented without the data necessary to be convincing. The evidentiary base is too thin, the argument too indirect, to support the sweeping generalizations. This does not mean the book is without significant contributions, only that it fails in its self-set awesome task. The strongest contribution is to challenge, as Marxian anthropologists are simultaneously doing (Godelier, Meillasoux, et al.), the romanticized component of patron-client models of pre-capitalist societies, to which Popkin's "Hobbesian" village world of insecurity, inequality and deprivation is a corrective antidote. He rightly argues that peasant insurrections often attack "long standing practices . . . as arbitrary, capricious, or intolerable" (p. 247), rather than seeking to restore the traditional world (though the language here again paradoxically connotes a heavy moral component).

The Rational Peasant provides, then, a useful critique of patron-client models while proposing an alternative script or extended metaphor which does not seem necessary (power as "comparative political advantage"? activists as "political entrepreneurs"?). The rational choice paradigm, like its neoclassical antecedent, suffers because "rationality," like "utility," can cover an infinite range of behaviors and meanings, simultaneously explaining everything and nothing. For Popkin, the rational peasant is calculatingly self-interested, but simultaneously "altruistic," directed by "sense of duty and ethic," guided by "preferences and values," etc. This expansion (and dilution) of the concept is fortunate for the author, for otherwise a great deal of peasant—and human—behavior would be inexplicable.

RONALD J. HERRING

Northwestern University

Perspectives on Revolution and Evolution. Edited by Richard A. Preston. (Durham, N.C.: Duke University Press, 1979. Pp. xiv + 300. $14.75.)

In 1976, in connection with the celebration of the American Bicentennial, Duke University hosted the International Bicentennial Conference of American and Canadian scholars. Participants in the conference included noted scholars from both Canada and the United States. The papers presented to the 125 individuals in attendance at the conference focused on the comparative development of law, politics, economics and culture. According to Richard A. Preston, the goal of the sessions was to use Canada as a foil in an assessment of the extent to which the myth of revolution has shaped American development. The collection, *Perspectives on Revolution and Evolution,* contains the papers presented to the Bicentennial Conference.

Most of the analyses and conclusions put forward are now well known to students of Canadian and American development. For example, S. M. Lipset's examination of the conference's proceedings is centered on his estimation that the United States has developed in accordance with revolutionary myths; Canada in accordance with evolutionary ones. The consequence has been social development in the United States characterized by liberty, equality, and anti-statism; in Canada by elitism, statism and hierarchy.

In this analysis, Claude Bissell argues that unlike in the United States, where a liberal educational policy emerged some time ago, Canadian policy remained elitist until the 1950s and 1960s: the goal of education was to preserve tradition. He also suggests that until recently, the predominant characteristic of Canadian literature was its emulation of European models. The colonial features of American literature were shed long ago.

John Porter, in turn, deals with popular ideologies of the "melting pot" and the "mosaic." While the former has dominated American perspectives of past immigration, and the latter Canadian perceptions, both are inaccurate. By and large, the actual experience of immigrants in both societies has been similar.

Although most of the assembled articles do not extend the body of existing knowledge of Canadian and American differences, they do state, in a concise and easily read form, *some* of the main features of Canadian and American development. The collection of articles will be of interest, therefore, to those generally concerned with comparative developments and to students. On the other hand, if they are interested in a general overview of comparative developments, one of the major weaknesses of the book, for both types of readers, is that many articles are written in a way consistent with functionalist theory. They therefore display the shortcomings of this general approach. In Canada, at least, the most important examinations of Canadian development are currently being undertaken by Marxist leaning political economists, sociologists, and historians. None of these is represented. Indeed, the existence of other modes of comparative analysis is not even mentioned in passing.

In short, *Perspectives on Revolution and Evolution* represents a handy and easily read collection of articles on Canada and American development. Many of them are functionalist in orientation. However, the collection does not cover the entire range of outlooks from which comparative investigations can be undertaken. This observation is perhaps more of a caution to prospective readers than a criticism of the book itself.

J. PAUL GRAYSON

York University, Toronto

Can Government Go Bankrupt? By Richard Rose and Guy Peters. (New York: Basic Books, 1978. Pp. xv + 283. $12.50.)

Over the last two decades most industrialized democracies have substantially expanded government spending as a proportion of the GNP, reaching these amounts by the mid-1970s (all levels of government): Japan, 23 percent; Australia, 24 percent; Switzerland, 26 percent; United States, 28 percent; Italy, 29 percent; United Kingdom, 33 percent; Canada, 34 percent; Austria, Belgium, France, and Germany, 37 percent; Sweden, 43 percent; Netherlands and Denmark, 44 percent; and Norway, 46 percent. Increases in direct cash payments to individuals and families (transfers such as pensions, family assistance, medical care, and welfare) and social services account for the recent growth in spending. Since 1960 in the OECD countries, tax elasticities with respect to the gross domestic product have averaged 1.4 for households and 1.0 for corporations.

Can Governments Go Bankrupt? argues that such spending must slow down, lest "political bankruptcy" strike. The reasoning: (1) "Indifference to authority is now widespread in the home, in schools, and in religion. There is no reason why it cannot become manifest in politics as well" (p. 6). (2) Such indifference to authority, one apparent version of political bankruptcy, is caused by government mismanagement of the economy and especially by increases in government spending that lead to decreases in "take-home pay." (3) Because governments are spending more and economies are growing less, take-home pay is declining. Therefore, Rose and Peters assert, political bankruptcy looms. Their remedy is reduced government spending and the indexing of income tax brackets.

"Political bankruptcy" is a vague and flabby concept, even for political science. In this book sometimes it means the indifference of citizens toward the government (the authors make no assessments of how countries differ on indifference); sometimes it means rowdy behavior of citizens (tax evasion, strikes, terrorism); and sometimes it means whatever symbolic content the names "New York City" and "Italy" have for the authors. Political bankruptcy does not mean Vietnam or Watergate. Whatever it is, at any rate, it is to be avoided at nearly all cost—the authors approvingly quote the portentous Peter Jay, who said that "the very survival of democracy hangs by a gossamer threat; . . . the dread familiar cycle of multiplying ineffectual regulations, rampant inflation, falling employment, industrial decay, administrative breakdown, social and political chaos, the emergence of a strong man on the right, revolution, and a self-perpetuating nominally left-wing oligarchy" (p. 14).

A curious and unconventional economic indicator, "take-home pay," makes the modest analytical machinery go. For Rose and Peters, the national economic pie is divided into two distinct pieces: "the costs of public policy" and "take-home pay." If the pie stops growing or shrinks, then one or both pieces must stop growing or must shrink. Since, by hypothesis, declines in pay cause political bankruptcy, the choice is clear, at least to the authors—reduced government growth. The argument is no deeper than that, a kind of rehash of the "Laffer Curve." Apparently, in this model citizens do not have enough sense to look at their real disposable income (that is, income after taxes and inflation)—which of course includes take-home pay *plus* all those direct cash payments to individuals (counted only as the "costs of public policy")—in deciding whether to become indifferent or rowdy and to achieve a state of political bankruptcy. Furthermore, although immediately relevant to the issues at hand, multiplier effects are ignored.

The evidence consists mainly of a few lackadaisical economic time-series from OECD publications, particularly the McCracken Report, *Towards Full Employment and Price Stability* (1977), which is the most cited work in the book, other than the previous publications of Richard Rose. No attempt is made to link—as in a table, scatterplot or regression—assessments of political bankruptcy, government spending, and take-home pay. And no wonder, for in a critique of similar work by Rose and Peters, Ronald Rogowski reports higher levels of political discord in countries with *lower* rates of government spending.

Skepticism about the growth of the public sector has been advanced with far greater depth, sophistication, and clarity by many, including George Stigler, William Niskanen, Assar Lindbeck, the public choice crowd, and even the median *Public Interest* essayist. Among recent studies, markedly superior analysis of government spending is found in the McCracken Report and other OECD publications and in papers by David Cameron (*APSR*, December 1978) and Richard Musgrave (APSA convention, September 1979). For teaching purposes, samples of Cameron, McCracken, Musgrave, Okun, and Stigler provide a fine mix of evidence, a range of conventional ideology, and political-economic models dealing with the public sector.

Aaron Wildavsky is quoted on the jacket flap: "This splendid book is essential for understanding the crisis of governability in the welfare state." It is nothing of the kind; rather it is the sort of book that leads economists to look upon political science as political scientists look upon sociology.

EDWARD R. TUFTE

Yale University

Administrative Secrecy in Developed Countries. Edited by Donald C. Rowat. (New York: Columbia University Press, 1979. Pp. xxiv + 364. $20.00.)

This volume, a study of public access to administrative information in 12 economically advanced nations, is the seventh in a series of surveys of conflict between public and private interest, sponsored by the Committee of Studies in Administrative Procedures of the International Institute of Administrative Sciences. Included in this survey are the four Scandinavian countries, Belgium, France, United Kingdom, West Germany, Hungary, Yugoslavia, Canada, and the United States, with each article being contributed by a citizen of that state. The editor, Donald Rowat, provides a useful comparative summary for the 12 nations on the availability of administrative information to the public and the continuing restraints imposed by the tradition of secrecy. The editor also contributes a sprightly and informative article on Canada.

The emphasis is upon constitutional and statutory foundations for rights and limitations to administrative documents and information, with detailed legal citations for each country. Typical rights are: "General Rights of Public Access," "The Holders of Information," "The Recipients of Information," and "Information Procedures." An excellent index gives comparative administration and public law scholars access to the countries' legal framework for access and secrecy which would otherwise be difficult to obtain. For example, by use of the index, a researcher desiring to know the general character and extent of privacy safeguards in the contemporary developed nations can achieve that objective with minimal effort.

The general aim of the survey as conceived by the committee was "(a) to define the boundaries between warranted and unwarranted secrecy, and (b) to inquire into the lines on which a satisfactory boundary may be struck between (i) the legitimate requirements of science, industry and the general public, and (ii) the need, for quite specific and justifiable reasons, to keep certain information confidential or even strictly secret" (p. xxix). Without making what for some scholars would be arbitrary normative decisions, this is an impossible dream and no fault should be placed for the failure to achieve that objective.

The operative, implicit premise of all contributors is that administrative secrecy is bad—public access is good. Undoubtedly this sentiment is agreeable to the great majority of political scientists, including myself, but that does not mean that such a laudable attitude should be exempt from critical examination. Public access is a means of public control of administration and can be in effect participative. Politicians and bureaucrats, with the knowledge that their decisions and actions in administrative processes can be revealed for public scrutiny, possibly in a prejudicial and unfavorable light, will be prone to precautionary, self-protective behavior. Conceivably in this era of unanticipated, turbulent change, governmental administration as a molding and stabilizing element would lose a great deal of its effectiveness. Powerful interest groups, selfishly motivated, who probably need the right of access least, may be the primary beneficiaries of the expansion of the public right to administrative information. Michael J. Singer, reporting on the experience with the Freedom of Information Act in the United States, briefly explores the possible dangers of unequal access having this origin.

Donald Rowat points out that the 12 countries can be divided into two categories: those that provide a constitutional and statutory basis for a public right to administrative information and those that do not. He includes the Scandinavian countries, United States, Hungary and Yugoslavia in the first category, and the four Western European nations and Canada in the latter (p. 1). Many would question this classification of Hungary and Yugoslavia even with his qualification that the public's right which they extend is "general," which "is not the same thing as a specific right of public access to documents" (pp. 18–19). Both countries are in the Marxist-Leninist tradition which affirms an integrative relationship for the public with the state, making adversary and conflict situations between citizens and the state organs illegitimate. This concept is implicit in the articles for both countries with citizens' rights involving ideas different from those of the West. In their own terms, public access to information is deemed a necessity for improved state administration. The fact that West Germany, France, Belgium, United Kingdom, and Canada still adhere to the traditional principle of discretionary secrecy, with grudging concessions to public access, raises a question as to whether freedom of administrative information is a trend integral to lasting changes in the governments of the developed countries.

HERBERT G. WILCOX

West Virginia University

The Jewish Minority in the Soviet Union. By Thomas E. Sawyer. (Boulder, Colo.: Westview Press, 1979. Pp. xxii + 353. $22.50.)

With all that has been written about Jews in the USSR in recent years, it is difficult to find a comprehensive, balanced treatment of the issue, one which has become a factor in international relations as well as in Soviet domestic affairs. This book goes a considerable distance toward filling the gap. Making intelligent use of Western and Soviet materials, Thomas Sawyer surveys the demographic, cultural, and legal positions of Soviet Jews and analyzes Soviet theories and policies which shape the Jewish condition in the USSR. The author has not uncovered any new materials, nor has he provided a new conceptualization of the issue, but he has synthesized a great deal of material into a persuasive presentation. He treats extensively the issue of emigration and ponders its impact on those Jews who will remain in the Soviet Union. Sawyer's conclusion, amply borne out by his analysis, is that "the official Soviet policy position toward the Jews is fundamentally an ambivalent one" (p. 232): while according Jews the status of a nationality, and, in fact, making it impossible for them to cast off their Jewish identity, "Successive leaderships have adhered to a singular tragic policy objective—the complete destruction of Judaism, both ethnically and culturally."

While not disputing Sawyer's conclusion, one might have wished for a more strenuous attempt to understand Soviet motivations and the contradictions of Soviet policy. There is no doubt that irrational factors are at play, and the study would have benefited from more probing of the mentality which produces those factors. There is an excellent discussion of the legal aspects of human rights in the USSR generally and of the Jewish situation in particular, but a more extensive discussion of the social realities behind the legal facades is needed in order to understand the outcomes of the legal and political processes.

Extensive appendices include relevant materials from the 1970 census volumes which are presented in a most useful manner as well as translated excerpts from the most important Soviet and international legal documents containing materials directly relevant to the position of Jews. The bibliography is extensive and useful. The book is therefore very handy as a compilation and synthesis of historical and current materials relevant to Soviet Jewry. One may question some of the author's conclusions—for example, that the Jewish situation has had considerable influence on other nationalities and dissident groups—but Sawyer has performed a valuable service in bringing together in a sensible way so much material. Because of its rather narrow and atheoretical focus, the book will not be of much direct use to those interested in comparative analyses of ethnic relations, minority politics, or the ways in which multi-ethnic societies pursue the goal of national integration, but it does provide a reliable data base both for those interested in the general area of ethnic politics and certainly for those interested specifically in the case of Soviet Jewry.

ZVI GITELMAN

University of Michigan

Political Participation in Latin America, Vol. 2: **Politics and the Poor.** Edited by Mitchell A. Seligson and John A. Booth. (New York: Holmes & Meier, 1979. Pp. viii + 262. $24.50, cloth; $9.50, paper.)

If (following Max Weber) we consider government the "organized domination of society," then politics consists of the exercise and competitions of power to influence or control the government. A notion of power clearly is central to politics. Political participation, then, in this grand sense, has to do with sharing the exercise or pursuit of power. A problem arises, however, when one turns to the study of the political participation of the poor in Latin America, a problem illuminated in this interesting volume. For the rural and urban poor in Latin America almost never share, and seldom dare pursue, power in their societies.

The problem arises because it is possible to confuse behavior manifest in the sharing or pursuit of power with behavior which reflects an exclusion from the power arena: that is, political participation and political manipulation. The argument of several authors in this volume (Adams, Foreman, Varela) is that several others focus attention on behavior that is constrained and manipulated, and define it as political participation (Booth, Seligson, Fishel). The confusion thus created, the argument continues, leads us away from the real object of interest, the ways in which power is organized and exercised. This, Adams states (Ch. 2), is the proper quarry of the academic hunter:

> It is probably useful to view participation as being the spoor of the animal we seek and not the animal itself. What we observe in these things called "participation" are aspects, phases, reflections, imprints, residues, or signals about the nature of a much larger structure... (p. 9).

Adams then pointedly remarks that a "good hunter can tell a great deal about his quarry by a careful observation and evaluation of the spoor, but he need not become a spoor hunter" (p. 10).

Seligson and Booth define political participation as "behavior influencing or attempting to influence the distribution of public goods" (p. 4). Thus they include a number of studies in the volume focused on various types of benefit-seeking behaviors of individuals, groups, or communities, directed toward government. Given the clientelist nature of Latin American politics, it is unsurprising that there is much of this behavior among the rural and urban poor. But does this illuminate political participation? Or does it illustrate the fact that they are excluded from the exercise of power and left with ritual favor-seeking as the only form of behavior tolerated by those who do exercise it?

It is instructive that the studies which lead the reader closer to the prey are those concerned with protest behaviors—land invasions, unionization struggles, strikes, and demonstrations—and that the response of the authorities is almost uniformly coercive. That is, when the rural or urban poor do actively seek to share or influence the exercise of power in their societies, they are suppressed. The study of the Bolivian tin miner's strike by Greaves and Albó (Ch. 13) most directly focuses on this process, but it is also discussed and considered in Loveman's study of rural labor in Chile (Ch. 14), and indirectly touched on in Bourque and Warren's study of women's power in two Peruvian villages (Ch. 9).

There may be a number of reasonable explanations to account for two such widely diverging approaches to the study of political participation—one concerned with power, and the other with behaviors constrained by power. It is a fact that graduate training in political science in general has diminished the emphasis on political thought, which dwells on and analyzes the question of power, and increased the emphasis on political behavior in the past two decades. Thus, many young scholars are simply less sensitive to fundamental notions of power and its implications and are more sensitive to, and trained in, methods of the analysis of behaviors. The result often is close attention paid to survey data on voting, attitudes, SES indicators, and organizational activities, and less to the struggle to achieve, or influence, the exercise of power. Survey research is the path to illuminate the former; long-term participant observation the method to explain the latter. It is perhaps no coincidence that the primary critics of the lack of power analysis in this volume are anthropologists (Adams, Foreman, Varela), while their primary targets are young political scientists (Booth, Seligson, Fishel).

Aside from bringing to light this fascinating problem in the study of political participation, *Politics and the Poor* contains a number of worthwhile and informative field studies by such dependable scholars as Henry Landsberger (Ch. 7, with Bobby Giersich) covering political behavior (whether manipulated and ritual or participant depends on the reader's considered definition) in Mexico, Costa Rica, Ecuador, Bolivia, Brazil, Peru, Colombia, Guatemala, and Chile. This is neither a definitive nor particularly significant work. But it most certainly is an interesting one.

JOHN DUNCAN POWELL

Tufts University

States and Social Revolutions: A Comparative Analysis of France, Russia and China. By Theda Skocpol. (New York: Cambridge University Press, 1979. Pp. xvii + 407. $29.50, cloth; $7.95, paper.)

This book has several characteristics that I believe deserve the attention of social scientists.

The first is that its format and style are those of a doctoral dissertation. There are long passages "reviewing the literature," describing "what this book will do," and defending its method of approach. There are (at the end) 56 pages of notes, many containing commentaries on material rather remotely related to the subject. There are long quotations, sometimes of doubtful pertinence, in the body. There is an effort to touch all relevant bases, as in the brief reference to studies of the "backgrounds of revolutionary elites" (pp. 166–67). The concluding chapter of 10 pages makes substantive statements on 10 additional twentieth-century revolutions and suggests how future research can fruitfully be conducted. In trying to follow the book's argument, I found all this most distracting and most annoying.

The second characteristic is the book's use of key words. Although "define your terms" may be sophomoric more often than not, when the terms stand for principal concepts in statements that purport to explain important events, I for one want at least clarity and consistency in their employment. "Revolution," "revolt," "insurrection," "struggle," and "upheaval" apparently are used as stylistic variations (e.g., pp. 67, 112, 164). Sometimes words are used in a way almost the reverse of the normal, as when we read that the French "peasant revolution" stopped well short of "antiproperty revolts" (p. 127).

The book's central concept, "social revolution," is defined as a "rapid, basic transformation of a society's state and class structures, accompanied and in part carried through by class-based revolts from below" (p. 33)—indeed, "class struggles" are "centrally involved" in social revolutions (pp. 28–29). Later, additional necessary conditions of social revolution appear: national order is reestablished, "socioeconomic transformations wrought by the class upheavals from below" are consolidated, and the country's "power and autonomy over and against international competition" are enhanced (pp. 163–64). Hence a social revolution necessarily includes a change in "class structure" and this change (despite some hedging) results from "class struggle."

The first part of this definition is acceptable, given the common-sense meanings of "state" and "class": basic transformations in economic, social, and political relationships did occur in France, Russia, and China during the periods under consideration. If, however, the term "class" is to convey more than this and if, especially, these transformations resulted from "class struggle," the term must be defined. No such definition appears, however, because it is not required by the "structural approach" being taken (p. 16). The result is that class becomes a primitive concept that every reader is expected to understand intuitively.

The third, related issue is raised by the book's possession of a feature often found in neo-Marxism. As the Marxist analysis of revolution in terms of the replacement of one class by another has become increasingly irrelevant to the agrarian environments of a large proportion of the greatest recent social transformations, an effort has been made to "modernize" Marxism. One such effort—as in the book under review—focuses on other "causes" of revolution, but at the same time retains the idea that revolutions "centrally involve classes." Given this new focus, no attempt is made to discover classes and examine their behavior, and so their "central involvement" becomes a matter of stipulation and the label "class" is tacked on to the customary names of certain social groups. The present book's treatment of China exemplifies this practice: the gentry were a class, the "rural dominant class," and so were the peasants. Nonetheless, the book acknowledges that the profound social transformations in the Chinese countryside were "orchestrated" by the Communists, because the peasants (presumably as a class) could not make their own revolution (pp. 278–79). To use only normal language, to say only that the Communists were able to redistribute land and thus change rural society by politically dividing its population, eliminates rather than "reforms" Marxism. (As an additional precaution, the present work considers the land-reform program ending in 1952 to be the "basic transformation" in agrarian China; the communization of the Great Leap Forward is omitted.)

My final point concerns possible attempts, not only by neo-Marxists, to get a "new focus" on great events such as the French, Russian, and Chinese Revolutions. The "new focus" of the present book is upon the "state": social revolutions are to be analyzed, "from a structural perspective," by examining the history of the "potentially autonomous" state, as it collapsed prior to, and as it was reconstructed during, social revolutions (p. 14). We read that the traditional autocracies in France, Russia, and China could not cope with internal divisions and external challenges and thus the three states collapsed. There is some detailed argument attempting to generalize about these divisions and challenges, but little is accomplished because, among other difficulties, the Chinese equivalent of the *jacquerie* and the Black Repartition must be invented. Social revolutions by definition enhance the power of the state, but the evidence for this proposition is similarly suspect. Nonetheless, "states" did collapse in France, Russia, and China, and new "states" arose, at least eventually. All well and good; but even an adequate explanation of these events would not satisfy our curiosity about the revolutions. The question of why the collapse and rebuilding of the state were accompanied by profound social and economic changes cannot be avoided. A "new focus" may be, as it is here, only an examination of something peripheral, while the big problems are ignored or resolved by definition.

C. W. CASSINELLI

University of Washington

Television and Political Life: Studies in Six European Countries. Edited by Anthony Smith. (New York: St. Martin's Press, 1979. Pp. x + 261. $22.50.)

This is the first in a series of research studies into problems of freedom of expression commissioned by WSET (Writers' and Scholars Educational Trust). Contributors are Anthony Smith, Fabio Luca Cavazza, Alfred Grosser, Åke Ortmark, Antoine de Tarlé and Herman Wigbold.

Smith concludes that political broadcasting in Britain is subject to constraints which belong to its own institutional framework, that the BBC and the IBA employ a model of political environment which is of their own making—constructed out of precedents set in broadcasting's own history. De Tarlé explains how French television is but a reflection of certain recurring features of French political life. He describes the absence of compromise and how the public television service became a political weapon. Real debates between political figures continue to remain rare and regional stations continue to remain under the strict supervision of Paris (despite the change in 1974 in official doctrine, when the newly elected Giscard told the populace that television was "not the voice of France"). Cavazza writes that Italian television is likely to reflect the opinions and wishes of the political parties as long as Italy maintains its present party system. Grosser concludes that although the achievements of German television are great, a new conformism has crept in, characterized by a fear of reporting. Swedish television retains for Ortmark much of its unruliness and capriciousness; nonetheless, toleration is now extended by it to a subjectivity on the part of those who present programs that would have been unthinkable before the 1960s. Ortmark remains convinced that "journalistic freedom in television can go no further" (p. 188). Wigbold feels that Dutch television has performed well as a public service for the nation, expressing itself in a manner unheard of in most other countries.

Contributors to *Television and Political Life* exhibit varying degrees of uneasiness relative to the future of television in their countries. The French scene remains for de Tarlé one of controversy between politicians and television, illustrating the former's fundamental incomprehension of the latter. Ortmark expresses reservations about the absence in Sweden of sufficient funding, television's lack of interest-group support, and the general deterioration in the country of the climate for mass media. Wigbold notes in Holland the inroads made by increasingly commercial methods, increases in cultural stagnation and political inertia, and loss to the press by television of its leading role in public debate. Smith—who concedes that barriers constantly are being constructed in each of these six societies as a result of the interaction between the world of professional broadcasting and politics—tells his readers, nevertheless, that he is not very pessimistic, that none of his contributors believe that journalistic freedom in their countries is in a state of disaster, and that more and more politicians are beginning to realize that television ought to be allowed to get on with its journalistic tasks.

Unevenness is characteristic of *Television and Political Life;* nevertheless, the book supplies considerable information about a medium whose importance dwarfs that of most other media of information, a medium about which courses in comparative politics need to know more and more.

LOWELL G. NOONAN

California State University, Northridge

Soviet Criminologists and Criminal Policy: Specialists in Policy-Making. By Peter H. Solomon, Jr. (New York: Columbia University Press, 1978. Pp. x + 253. $15.00.)

The aim of this study is indicated by its subtitle. It is to contribute to our understanding of the policy-making process in the Soviet Union, specifically the role of specialists in this process, and thereby to help resolve the differences of opinion among Western analysts concerning the degree to which the Soviet policy-making process can be said to resemble or to differ from that in Western pluralist systems. To this end, Peter H. Solomon has provided an excellently researched case study of the participation of Soviet criminal law specialists and criminologists in the initiation, discussion and passage of a series of criminal law reforms. On this basis he concludes that the evidence supports the view expressed by Jerry Hough that there is a form of pluralism in decision making in the USSR.

Contrary to the image of the Soviet system as one in which the political leaders dominate policy formation and in which officials and experts participate at best on a limited scale, Solomon finds that there is regular debate and discussion among leaders, officials and specialists in which the political leaders often act only as brokers to resolve differences. Neither the character of the criminologists' participation in terms of its scope, quality and effect, nor the political constraints on their capacity to influence policy appears to be significantly different from what is found in the U.S. or Britain. This situation, Solomon adds, is not a new phenomenon in the Soviet Union, and therefore the recent Western awareness of specialist participation in Soviet policy making can be attributed primarily to changes in the perceptions of Western analysts rather than to changes within the Soviet system.

Solomon arrives at these conclusions, which are in distinct contrast to those drawn by Zbigniew Brzezinski and Samuel Huntington in *Political Power USA/USSR*, by employing the same criteria of participation as were used in that earlier work. He does this first in examining the activities of criminal law specialists during the Stalin and Khrushchev periods and effectively demonstrates that there was meaningful participation even under Stalin and that it expanded and became institutionalized after 1956. This is followed by a more detailed account of the role of these specialists in the revision of laws dealing with delinquency, alcoholism, hooliganism, parole and recidivism during the middle and late 1960s. The research on this section, which is the core of the study, included interviews with a number of Soviet criminologists from whom Solomon was able to gain invaluable insights into the behind-the-scenes contacts which went on throughout the process of formulating the policies and converting them into law. Of considerable interest here is the discussion of ad hoc working commissions composed of officials and experts which were set up by the Supreme Soviet to prepare draft laws and which provided important points of access for experts concerned with the particular questions at hand. Little has been known or written about these before. The final section of the book is devoted to a systematic, and somewhat repetitious, reexamination of the findings of the previous sections, as well as to a more interesting discussion of their implications for an understanding of the nature of Soviet policy making.

It may be an indication of the changes in Western perceptions of the Soviet Union, as Solomon suggests, that I found some of his conclusions about the role of experts in policy making neither as surprising nor as significant as he seems to believe them to be. Many of the specialists whose activities he examines work in research institutes set up under the central law-enforcement agencies. The fact that the top Soviet leaders do not dream up and work out all policies but must depend on advice and information from officials and experts working in specialized party and state organizations and their associated research institutes is hardly shocking. Neither is the fact that on relatively specialized questions the political leaders would defer to the judgment of such experts and would only interfere to resolve differences. To say that bureaucrats and legal, scientific or other experts perform a similar role in the United States or Britain, or for that matter in Nazi Germany or Brazil, comes as no surprise and does not address the issue of the amount of pluralism found in the various systems. By explicitly rejecting an interest-group approach and by admitting at the end of the book that there still are significant differences between what Hough calls the "institutional pluralism" of the USSR and the "democratic pluralism" of the U.S. and Britain, Solomon in essence does accept the fact that his comparative focus is relatively restricted. At the same time, this is an important book for anyone interested in questions of Soviet law and policy making and sets a standard of research which will be hard to match.

DAVID L. WILLIAMS

Ohio University

Transkei's Half Loaf: Race Separatism in South Africa. By Newell M. Stultz. (New Haven, Conn.: Yale University Press, 1979. Pp. xv + 183. $16.00.)

Newell Stultz offers in this slim and succinct volume the first major appraisal of Transkei since it was bundled by South Africa into a nominal sovereignty recognized only by Pretoria. Transkei "independence," while it is bitterly rejected as a fraudulent maneuver by other African states, is nonetheless a significant development in the South African context. The flag-raising in Transkei in 1975 was the symbolic culmination of the master strategy of *apartheid* and separate development pursued by the ruling Nationalist party in South Africa since it achieved power in 1948. The 13 percent of the land surface formerly classified as African reserves had been redefined as ethnic "homelands," whose destiny was an eventual sovereignty which, to Nationalist white ideologues, constituted a "final solution" to the racial issue through assignment of all blacks as citizens of these ten impoverished entities.

In reality Stultz amply demonstrates what even the South Africa authorities now appear to recognize, that the homelands cannot possibly serve as a receptacle for all Africans, or even very many beyond their existing inhabitants. However, the state structures being created provide an institutional base for a new element among African leaders, whose importance should not be underestimated. The structural, material, and legal resources available to the homeland elite are far from negligible, and make them an indisputable factor in future developments.

Stultz usefully situates his analysis within the unfolding debate over "cultural pluralism" and "consociationalism" which have become surprisingly fashionable terms among those in the white South African community who seek some escape from the racist trap in which the republic is ensnared. He carefully explores the important issues of what benefits have been conferred upon whom by Transkei separation, and the extent and limits of its nominal independence. Possible future implications of the Transkei formula are thoughtfully examined.

To simplify the Stültz argument somewhat, we may identify three main components of Transkeian society: the state elite (politicians, chiefs, civil servants), urban Xhosa whose (often distant) ancestry traces to Transkei, and the peasant mass. The prime beneficiaries of Transkei sovereignty are evidently the first group; they gain an arena where the disabling apartheid structures are dismantled, and where quite rewarding opportunities are open. Both chiefs and civil servants have proliferated remarkably, and enjoyed swiftly rising remuneration; the Transkei budget rose 75 percent in 1977–78, mainly reflective of the state-expansion dynamic. The major losers are the urban Xhosa, who not only derive zero benefit, but find their residential status rendered precarious by the Pretoria view (not shared by Transkei authorities) that they automatically become Transkei citizens with no rights in the Republic. This group is no less than one-third of hypothetical Transkeians (and a much larger fraction of several other homeland populations). The third group, the peasants, have neither gained nor lost much. Elimination of discrimination has little impact, as they are unlikely to patronize institutions with racial barriers; there seems little prospect in an improvement in their livelihood.

The Transkei political system clearly reflects these facts. The dominant political organization, the Transkei National Independence party, is a lifeless creature of the state apparatus, with few real members, little meaningful following among the rural mass, and none among urban Xhosa. Transkei premier Kaiser Mantanzima is a ruler in the personalist-authoritarian mold—not simply a Pretoria toady, but no charismatic leader either.

Stultz's conclusions are puzzling. While he is persuasive in arguing that the new Transkei status will have some impact in the future evolution of South Africa, his view that the international quarantine of this South African hostage state should be ended seems, at least to me, to be contradicted by the powerfully negative evidence which he has so dispassionately assembled. Even more mystifying is his parting salute to Mantanzima as a man who "has delivered more in the way of *actual* African emancipation from white oppression in Southern Africa than any other individual in history to date" (p. 167). While many may part company on the conclusions, all readers concerned with southern Africa will benefit from perusal of this trim and valuable book.

M. CRAWFORD YOUNG

University of Wisconsin, Madison

The Communist Parties of Western Europe: A Comparative Study. By R. Neal Tannahill. (Westport, Conn.: Greenwood Press, 1978. Pp. xvi + 299. $18.95.)

This study of Communist parties in 18 Western European countries is well researched and well documented. It is a storehouse of information, esoteric as well as centrally significant, about large, medium-sized, small and tiny Communist parties alike. Much of the book follows an analytical mode, enabling the author to pose numerous hypotheses about the factors which make for similarities and differences between CPs in doctrine, style and tactics. In addition to being a source of information and hypotheses, the book contains an extensive bibliography and useful tables of election statistics.

In some ways Neal Tannahill's approach reminds one of Duverger's classic study of political parties, in that it attempts to assimilate a mass of information about similar phenomena and produce propositions which can subsequently be tested. Among the more promising propositions are those dealing with the age-structures of CPs. It is hypothesized that CPs whose memberships are still dominated by pre-1956 party entrants tend to be more "conservative" than those in which younger members are more numerous. A similar hypothesis is made regarding CPs whose memberships and/or electorates are heavily working-class in composition, the CP of Luxembourg, one of the most pro-Moscow parties in the set, serving as a good example.

Unfortunately, the work suffers from the much-criticized Duverger tendency toward ad hoc comparisons, lacking in systematic examination of the full range of data. Such two-way comparisons as those in chapter 10, between the CPs of Iceland and Luxembourg, of Spain and Portugal, and of Italy and France, are perhaps fruitful for suggesting further research; but without drawing them together with parallel observations for the whole range of countries, one cannot really be certain what one has. If, in a given comparison, the other relevant factors had been controlled, then we might have been more impressed by the observation that Party A, the more "conservative," has an older membership than Party B; or that Party X, the more Moscow-oriented, had most of its leadership spend their years of exile in Moscow or elsewhere in Eastern Europe.

More serious is the fact that the author failed to do what Duverger so brilliantly did: make systematic *classifications* of parties on the basis of specified criteria. This is most blatant in the absence of any clearly specified *dependent variable.* As far as can be judged, the author is seeking to account for similarities and differences in the behavior of CPs on two dimensions: (1) inter-party relations within the world Communist movement, and (2) domestic politics within the Western European countries. On the former dimension, parties are compared in terms of their relations with Moscow, ranging from the aloofness of the Icelandic CP to the subservience of, among others, the Austrian CP. On the domestic politics dimension there seems to be a less central focus, but parties seem to group themselves according to (1) their relations with other parties closer to the center of the spectrum, and (2) the varying degrees of doctrinal flexibility which tactical considerations dictate.

Toward the end of the book Tannahill distinguishes between two "ideal-types," the *Neo-Stalinist* party and the *Eurocommunist* party. Apart from the fact that, in regarding these as ideal-types, he avoids having to classify all 18 parties as one or another, what is striking is that the "types" appear to combine elements of the two above-mentioned dimensions, without any effort having been made to explore the implications of doing so. Nor have such implications been recognized at any earlier point in the book. But is a party which is neo-Stalinist in its adherence to a Soviet line internationally (vis-à-vis China, for example) necessarily neo-Stalinist in its approach to domestic politics? With reference to the latter, Tannahill seems to regard neo-Stalinist parties as "conservative." If so, then how does one deal with the domestic record of the Portuguese CP in the mid-1970s? Presumably, also, both the Italian and the French CPs are leading Eurocommunist parties, yet the PCF, presumably the more "conservative," espouses domestic programs which are further to the left than those of the PCI. Both, incidentally, are labeled "nationalistic," in comparison with the neo-Stalinist type; yet they show notable differences in their orientations to European "supranationalism." In short, a massive data-gathering effort remains largely unassimilated in this work.

DAVID M. WOOD

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Nasser and His Generation. By P. J. Vatikiotis. (New York: St. Martin's Press, 1978. Pp. 375. $22.50.)

Despite the book's title, P. J. Vatikiotis, an American-trained professor of politics at London University's School of Oriental and African Studies, reveals more about Gamal 'Abd al-Nasir than about his generation. The work is essentially a clinical examination of the anatomy of Nasser as schoolboy soldier and ruler, and an evaluation of his long-range influence on Egypt. As such it is first-rate, particularly for an English-reading audience. Vatikiotis draws most of his evidence from the large and steadily growing body of relevant books and articles in the Arabic daily press and periodicals. This is the author's second full-scale inquiry into the military overturn of the Egyptian monarchy. He wrote the first one two decades ago, while Nasser was still developing and consolidating his style of personal rule. In between, he also wrote a superior history of Egypt after the Napoleonic occupation in 1798, so that he sees the postwar political development in perspective.

Nasser, the author observes, presided over the third "epochal" transformation of Egypt since 1800. The first mutation is attributed to Mehmed 'Ali Pasha (1805–49), an Ottoman soldier of fortune who "ruthlessly forged the basic structures of a modern state," leaning in the process on an army modeled on those of Europe with the help of advisers and mercenaries from the continent. The second occurred under the British occupation (1882–1922), when the proconsuls modernized the "fiscal and judicial administration" through a British-assisted bureaucracy, "made Egyptian agriculture more efficient and productive, and completed the incorporation of the Egyptian agricultural economy into the European market economy and patterns of trade." Nasser's break with the past came not immediately after the military seizure of power in 1952, but four years later, when he rose to the presidency of the republic which he held until his death in September 1970. In that decade and a half he fashioned "the bases and framework of a popular despotism," through which he sought to reshape the society and the economy as well as the polity, using "a new technocratic-military state bureaucracy" and Soviet advisers as his main struments of creativity (p. 358).

How do you account for the socialization of Nasser and his colleagues to political intrigue that led them to plan and to carry out the destruction of a dynasty that had lost touch with reality? How and why did Nasser convert a collegial political executive into personal rule? How did he continue to project the image of a populist while hardening his grip on the political management of Egypt through the progressive displacement of his original partners, even after the military disaster in the Six-Day War with Israel in June 1967? To these questions the author addresses his latest work.

Only the opening section assesses the political conditioning of the members of the junta, when they were still teenagers and long before they could have aspired to military careers, to say nothing of organizing a clandestine Free Officers movement. Vatikiotis traces their initial politicization to the anti-imperialist radicalism that sped through the high schools of Cairo and the cities of the Delta in Lower (northern) Egypt in the 1930s. The author enriches the available Arabic materials with supportive evidence from the Public Record Office (London). All those who later became Free Officers graduated between 1938 and 1942 from the military academy in Cairo, for which the 1936 Anglo-Egyptian treaty had provided. The author discovered that this baker's dozen of junior officers, mainly recruited from the lower middle class, had belonged to or sympathized with one or another of the radical societies—chiefly, the Misr al-Fatat or Young Egypt Society (that grew in 1940 into the National Islamic party and a decade later into the Egyptian Socialist party), the religious fundamentalist Muslim Brethren, and a clutch of Marxist and communist groups. Vatikiotis found an "astounding similarity in style, argument, idiom and preoccupation between . . . [the Young Egypt movement] and that of the Free Officers, including Nasser's own utterances in later years" (p. 62).

The author feels, instructively, that his thorough screening of the Arabic sources still fails to yield incontrovertible evidence on the origins and development of the military conspiracy that toppled the monarchy. He seems more secure when he explores in analytical detail Nasser's removal of his rivals in an irrepressible search, not for material wealth, but for "the seizure, exercise and retention of power as the sole ruler of Egypt," while continuing nevertheless to remain "a gentle, reticent, unassuming family man" (p. 314). By the last year of his life, in addition to the presidency, Nasser served as prime minister and did not assign to others the leadership of the exclusive government-organized mass party, the Arab Socialist Union. Moreover, as commander in chief of the armed forces, following the debacle of June 1967, he even personally supervised the promotion of all officers in the three branches of the armed forces to the equivalent of colonel and higher. "The man therefore who, in less than a generation, became the oracle of Egypt's destiny and of Arab revolution at mid-century," Vatikiotis concludes (pp. 363–64),

> was a soldier by chance, a politician by instinct, and a conspirator by ability and inclination. He contributed little to soldiering or politics. But he elevated the more native art of conspiracy to new technical heights. At the end of the day, however, he was by formation, tradition and force of circumstances an Egyptian despot.

The author's handling of medical and psychological analysis is amateurish. The book is also marred by a loose structure and occasional redundancy, perhaps unavoidable in view of the parallel vertical assessments of Nasser's unfolding political style. The publisher may perhaps bear more blame than the author for the slipshod copy-editing and proofreading. Still, on balance, this is the most reflective and mature work on the first president of republican Egypt.

J. C. HUREWITZ

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International Politics

Terrorism: Theory and Practice. Edited by Yonah Alexander, David Carlton, and Paul Wilkinson. (Boulder, Colo.: Westview Press, 1979. Pp. xiii + 280. $20.00.)

There has been no shortage of recent collections on terrorism in the past few years (indeed this is at least the third edited volume by Yonah Alexander himself). Most of the essays in this volume were presented at two conferences organized by the editors in 1976 and 1977. As with too many sets of conference papers, these essays are very uneven in quality. And, as too often with edited collections, the essays are without overriding coherence and do not adequately cover the important issues that terrorism and its study raise.

The volume is in five unequal parts. The first, "overview," contains a single article by Russell, Barber and Miller which neither overviews the volume nor the phenomena of terrorism. Rather, it is an interesting discussion of what terrorists *might* do in the future employing what the authors term "traditional," "analytical," "speculative" and "crisis" management approaches. The second, "The Causes," contains two chapters. Paul Wilkinson discusses the limitations of social science theory, particularly that based on psychological or biological aggression theory, for understanding the causes of violence. However, he says nothing about terrorism or the connection between violence in general and terrorism in particular. Amy Redlich focuses on the contagion aspects of terrorism as societies are penetrated by "volatile information." She suggests that open societies which have populations identifying with outside sources are most sensitive to external influences. She provides examples but does not adequately develop causal linkages for her thesis. Part 3, "The Perpetrators," features a single article, Paul Wilkinson's brief analysis of four types of terrorist movements: nationalist, ideological, exile and transnational gangs. Using the framework he developed previously for studying social movements in general, Wilkinson discusses strategies, tactics, organization and success rates of terrorists, concluding with an implied plea for a diminution of freedom in open societies because authoritarian societies have greater success rates against terrorists than do liberal societies. Part 4, "The Issues," has four articles. The first, Alan O'Day's analysis of Northern Ireland, is more concerned with cultural history of Republican violence in Ireland than it is about terrorism. Furthermore, he analyzes activities of the republican movement without attending to the interaction among the state, the "Protestants" and the "Republicans" (his division the actors). After all that has been written on Northern Ireland in the past decade, the limitations of this analysis should be obvious. Most specialists recognize that an understanding of the situation requires analysis of the role of all the major actors in the process and that no one party to the conflict can be seen as merely responsive in its behavior.

Abraham Miller's contribution on hostage negotiations is easily the most worthwhile in the collection. It is a clear, thoughtful analysis of the impact of terrorism on the hostages and the process of negotiating their safety. He concerns himself with the chances of success for hostages, negotiations and the terrorists themselves, and the role played by the public and the media in the process. He counsels caution and increased awareness of the difficulties faced by the hostages as well as the negotiators and makes a plea to avoid quick emotional and violent responses. Yonah Alexander's short article on the media covers much of the same ground already plowed by Redlich in part 2 but calls additionally for a research effort to study the interaction of terrorism, the media, and the police. L. C. Green closes out the issues section with an analysis of how the principle of self-determination has served to legalize terrorism in the current practice of international law. He concludes that there is thus little chance that international organizations will be able to develop or employ international law which will provide measures to control terrorism.

The final section, "The Future," consists of two articles. In the first, David Carlton examines the effects of "technological innovations," "self-imposed restraints," "countermeasures," and "international variables" on the future characteristics of terrorism. However, terrorism is defined so broadly (all substate violence that is political in character) and the terms of the discussion are so general and filled with assessments that the author suggests might, may, or perhaps could happen, that the rigorous speculative analysis called for by Russell, Barber and Miller in chapter 1 does not emerge. Finally, the last selection, the revised text of a speech by Robert Friedlander, confronts the problem by suggesting that we terrorize the terrorists by raising the penalties of terrorist actions. Concomitantly, Friedlander suggests that we explore reducing individual freedoms for the purpose of increasing societal security. He offers no evidence that this will reduce terrorism, but rather turns his frustration concerning the limits of current international law and the current success rate of terrorists into a challenge of the will.

All in all, this is not a particularly useful volume. Alexander's previous two volumes and Wilkinson's two monographs provide more worthwhile material.

MICHAEL STOHL

Purdue University

Strategy and Ethnocentrism. By Ken Booth. (New York: Holmes and Meier, 1979. Pp. 191. $21.50.)

Twenty years ago George Kennan, perhaps the dean of American strategists, observed in his book *Russia and the West Under Lenin and Stalin* (Little, Brown, 1960) that one of the reasons why the peace treaty that ended World War I was fundamentally a failure, was that Western negotiators proved not "able to envisage and apprehend the spirit of another society." This kind of error is called ethnocentrism, and it is the burden of Ken Booth's book that strategists, present and past, are gravely hobbled by it. Writing, as he says, "largely about failure and fault within the strategic community" (p. 178), he finds ethnocentrism operating there in many ways at many levels, at considerable cost and danger to us all.

Just which strategists are being criticized this way is a bit unclear. Booth never tells us explicitly, and seems to shift his fire among several targets without notice. Most American political scientists would assume that strategists are primarily the civilian "defense intellectuals" who have played such an active role in creating the conceptual apparatus of American strategic policy, and at times Booth does seem to mean these people. But an offhand opinion that most American strategists are "lower middle class" and many other indications seem to suggest that, in much of the book, he has in mind professional military officers of the kind that prepare the services' contingency plans. A later chapter is apparently addressed to university teachers and scattered remarks elsewhere take policy makers to task. Booth's flexible targeting allows him to get off the maximum number of rounds without noting that his criticisms of one group often apply partially or not at all to another.

Still, what he is criticizing fundamentally is a style of thought which, he argues, infects to some degree most people doing strategy in any sense. There is real validity in this, even though he tends to define "the strategic paradigm" in a narrow way, thereby making his argument easy. Strategic thinking does require enemies, does promote "worst case" analysis, does encourage one to "see all roads of aggression [as] leading to one's own homeland" (p. 36). While exploring his theme Booth makes some telling points. Ethnocentrism, for instance, leads the West to judge Soviet sea power from its own, Mahan-derived viewpoint, and to misunderstand the *Soviet* use of terms like "detente" and "peaceful coexistence" which to the Kremlin mean conflict in another form.

Unfortunately, the power that Booth's argument merits is dissipated by a writing style that may seem tedious even to those predisposed to be receptive. He does have the gift of the pithy phrase: for example, "Whom decision-makers wish to ignore, they first give the reputation of devil's advocate" (p. 165). Otherwise the style, particularly as the book wears on, is meandering, repetitive and quite tendentious.

Substantively, too, the argument is flawed. Much of the concrete evidence is given so summarily that the reader cannot determine the relative importance of ethnocentrism in a policy that miscarried, and is reduced to taking the author's word for it. Some of the argument amounts to little more than a reworking of the differences between high- and low-quality strategic analysis, about which the better professional strategists would be the first to agree. And few useful ideas are offered for ameliorating the deficiencies Booth criticizes so lavishly. There are hardly any specific suggestions for the improvement of governmental policy-making methods; and when, near the end, he prescribes the great empathy, imagination, and command of multiple disciplines and skills of the adequate strategist, he does not seriously consider the difficulties and costs of developing such people. Others, too, have wished for Renaissance men to guide our international destiny.

In essence Booth is making the same case—that good strategy depends on seeing things through the opponent's eyes—that has been made (as he partially acknowledges) by Kennan and many other great strategists, and again more recently and systematically by Robert Jervis, Alexander George, and other political scientists. As Booth wisely points out, it is an idea that commands almost universal assent, but many who think they understand it, in fact greatly underestimate how pervasive and how serious is the failure to achieve it. Whether this flawed book will much improve things is uncertain.

RICHARD SMOKE

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Studies in Crisis Behavior. Edited by Michael Brecher. (New Brunswick, N.J.: Transaction Books, 1979. Pp. 384. $12.95.)

This book is a collection of 13 articles constituting a preliminary report of the International Crisis Behavior projct, an ongoing study of the international crisis behavior of individual states under the supervision of the editor. The project builds upon crisis research undertaken during the 1960s by Charles McClelland, Charles Hermann, the Stanford Studies of Conflict and Integration, and others. The focus is upon the decision and other cognitive processes of national leaders within a single state and not on the interactive processes of two or more states in a conflict system. The policies and behaviors of other states are treated as inputs into the actor system.

Michael Brecher identifies four necessary and sufficient conditions for a crisis situation: (1) a change in a state's external or internal environment which generates (2) a threat to basic values, with a simultaneous or subsequent (3) high probability of involvement in military hostilities, and the awareness of (4) a finite time for response to the external value threat. Like several others in the crisis field, Brecher eliminates the element of surprise as a necessary element of crisis.

In an opening essay Brecher presents a model, three sets of research questions (22 questions in all) pertaining to threat, time and probability of war, and three tables defining the attributes of the crisis actor, the dimensions of the crisis, and the characteristics of the crisis decisional unit. Subsequent chapters report on "vertical" and "horizontal" studies of 469 actor cases, including 79 intra-war crises, between 1938 and 1973. The vertical studies deal with individual cases representing one or another of the boundary-free typologies in the above-mentioned tables. The horizontal studies amount to comparisons across dimensions, attitudes and characteristics.

The first chapter provides a theoretical approach to international crisis behavior with refinements in aspects of the traditional literature. Reporting on the case studies, the next nine chapters include initial findings from investigations of the United States in the Berlin crisis of 1948 and the Syria-Jordan confrontation of 1970; the USSR in the Czech crisis of 1968; Holland prior to the 1940 German invasion; Britain in the Cyprus conflict; Israel in the 1973 Middle Eastern crisis; and India in its 1962 border crisis with China.

In the eleventh chapter, Brecher presents a summary of findings from the vertical case studies, and in the twelfth he and Hanan Naveh report on some preliminary findings of the first horizontal study—patterns of international crises in the Middle East, 1938–1975.

Implicit in the overall findings, but not adequately addressed, are the phenomena of equi- (and multi-) finality, which are characteristic of human affairs and a challenge to rigorous theoreticians in all the social sciences, e.g., the same "causal path" can lead to different outcomes, and the same outcome can be reached by different "causal paths." This consideration can be an embarrassment to any student of crisis, but especially one concerned with crisis management.

Another difficult challenge in the study of crisis decision making (in all decision making, indeed) is the issue of rationality. In chapter 13, Janice Stein addresses the following questions: Can decision makers be rational? Should they be? And what are some of the attendant problems that the analyst cannot avoid confronting? The assumption is widespread that the case for a comprehensive decision strategy rests in large part on a comprehensive concept of rationality. But how rational are decision makers, in fact, or anyone else? From a phenomenological perspective, decision makers define the boundaries of their problems through their perceptual networks. What is real is only what the decision maker considers to be real, and hence an "objective" environment is ultimately unknowable and therefore irrelevant.

Similarly, cognitive psychologists have argued that rational decision making, however desirable, is not the empirically dominant mechanism of choice. Basic cognitive structures constrain the human capacity for complex calculations and reasoned argument. Foreign policy makers are limited not only by their unconscious mental processes and inadequate mental equipment, but also by the complex interdependencies of their environment. What is rational for one level of organization, moreover, is not necessarily rational for a component organization (or individual), and vice versa. Nor has any precise threshold ever been established between rational and irrational processes. Within broadly defined limits, Stein concludes analysts must use their own judgment to distinguish rational from irrational processes.

In the last chapter, Raymond Tanter presents a critical assessment of the international crisis literature. Scholarly research on crisis decision making has been remarkably cumulative, he concludes, and it "abounds with significant generalizations." But the impact of crisis on the decision maker's search, revision of information, and evaluation of options and choice is still "not clear." Nor is it clear how much confidence should be placed in the knowledge so far available for use in crisis management.

All in all, this is a book which anyone in the field of international crisis and crisis decision making will want to keep within easy reach.

ROBERT C. NORTH

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Europe Between the Super-Powers: The Enduring Balance. By A. W. DePorte. (New Haven: Yale University Press, 1979. Pp. xiv + 256. $18.50.)

A. W. DePorte is a member of the Policy Planning Staff of the U.S. Department of State, and he completed his book as a fellow at the Council on Foreign Relations during 1976–77. While the book "does not necessarily reflect the policy or views of the department," it should nevertheless be read with the knowledge that the author has enjoyed a unique vantage point from which to observe European politics.

The conclusions presented here are thought-provoking, and that is one mark of a successful critical essay. For any scholar or lay person concerned with European affairs, the book contains challenging conclusions, particularly concerning the future of a continent divided by two opposing ideological systems and by two opposing military alliances; namely, that from the "decline and death of the classical European state system" (p. vii) has emerged "a bipolar system dominated by the two superpowers which includes the states of Europe but is no longer centered on Europe . . . and [could] well survive indefinitely because it serves the interests of so many states, and particularly of those with power to change things" (p. vii).

His conclusion is preceded by a discussion of the European state system as it had developed to 1945, and in due course by an analysis of the "new state system" that developed during the "cold war" and which replaced "the one finally swept away" in 1945 (p. 115). This "new state system" was the creation of "two organized blocs in Europe" which provided its structure, but not its stability:

> For that there had to be a tenable solution of the German problem which had caused the breakdown of the prior European system and led straight to the dual hegemony (even in antagonism) of the United States and the Soviet Union. Neither of these powers intended at first to bring about the German solution which, in

> fact, they did bring about. Yet almost everything they did with respect to each other led to it with as great a precision as if they had planned it. [!] The completion of the division of Germany, the closing out of other options, is the most striking outcome of the cold war—except for the emergence of the two-bloc system whose stability first required and has since maintained that division (p. 141).

The author makes clear that the stable division of Europe, consolidated by "the German and European settlements reached in 1955," does not "exalt stability as a value over self-determination" (p. 165). But the reader is left wondering why the author does not devote more attention—in fact, he devotes scarcely any at all—to the values of self-determination, of freedom, and of individual liberty held by the peoples of divided Europe. His almost total emphasis is on the claim "that the European system came to be what it is because, to paraphrase Thiers, it divided least those with power to affect it" (p. 165).

In the author's effort to present objectively the factors that have produced Europe's division, he has neglected an analysis of the serious questions posed by the division of Germany and the institution of dictatorial governments in Eastern Europe. What could be the explanation for so glaring an omission? It was made necessary by DePorte's strong inclination to conclude—in advance—that "the state system which up to now has so well reflected the true balance of power in Europe that has emerged from the crises of the twentieth century has a strong lease on the future" (p. 244). This conclusion may in time prove to be correct. But whether it is a positive consequence of the "new state system" is quite another matter. The "new state system" may have "effectively performed its basic tasks for the member states—survival and maintenance of peace—for three decades" (p. xiii), but if this is so, it has been at the expense of the liberty and freedom of East Germany and of the Eastern Europeans.

Another, and equally interesting point to which the author could have devoted greater attention, is whether the Europeans consider the system "to be normal" (p. xiv). And finally, it is in one sense completely irrelevant, and in another the height of effrontery, to assert that the "new state system" is "better, even, for the 'greatest good of the greatest number' of the peoples of Europe in terms of the liberal democratic values which I hold" (p. xi).

DePorte's point is well taken that "it is not clear, considering the course of World War II, or even World War I, that it would have been possible to bring about a state of affairs in Europe that would have been better than the one we have known since 1945" (p. xi). So far so good, but the author, in his somewhat fatuous interpretation, takes on more responsibility that he is entitled to assume. Even if the system is "stable," as DePorte maintains it to be, is it not imperative to ask whether the price the Europeans above all are paying for that stability is acceptable to them? On the basis of historical experience, and if the democratic idea that had its birth in Europe remains alive in the hearts and minds of the Europeans, the "stability" of Europe between the superpowers does not represent an "enduring balance." It does represent a provisorium, and that places a large question mark in Europe's future.

DENNIS L. BARK

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The Enclosure of Ocean Resources: Economics and the Law of the Sea. By Ross D. Eckert. (Stanford, Calif.: Hoover Institution Press, 1979. Pp. xvi + 390. $16.95.)

Ross Eckert has written an important book on trends during the last half of the twentieth century which have turned over vast areas of the seas, formerly governed by the historic "freedom-of-the-seas" doctrine, to state jurisdiction. His purpose is to analyze the effect of the change from high seas or communal ownership of the seas resources to a regime of more exclusive rights, and to show that this change has been an economic response to basic changes in world economic demand for the sea's resources, and the development of technologies for extracting these goods. Although many writers have been highly critical of the extension of coastal state jurisdiction over ocean resources, Eckert forcefully argues that economic analysis indicates that enclosures will result in a more efficient exploitation of these resources.

Prior to enclosure, each state and its citizens has a private right to a resource (fish, hydrocarbons, etc.) once it is taken, but only a communal right to the same resource before it is seized. The tendency is to convert one's rights into the most valuable form; resources owned under communal arrangements will be converted into resources owned privately—that is, they will fish or drill for oil in order to establish private rights over the resource. The instability in the communal right system becomes critical when changes in demand or technology make the communally owned resource more valuable than it had been. The result is competition to acquire private rights by extensive fishing which induces efficiency losses that stem from overfishing. The emergence of state-owned property rights enhances the value of conservation measures to obtain the maximum sustainable yield, and in reducing pollution and other factors that would reduce the revenues derived from the resource.

Each chapter in part 1 expertly develops the new property arrangements for ocean resources that have been developed by coastal state enclosures or other international agreements, and Eckert develops the economic consequences for efficient resource use concerning coastal states': extending territorial seas to 12 miles; exclusive control over hydrocarbon exploitation and the mechanisms for sharing some derived revenues with the international community; control over fish and other living resources in coastal areas along with the problem of dividing access rights between coastal nations and other countries that have historically fished coastal waters; increased control over activities that cause marine pollution; requiring prior consent for basic scientific research; and, finally, the implications of creating an international regime to control access to areas beyond the present boundaries of national resource jurisdiction. Eckert's analysis is empirical in that he puts forward the economic theory and then notes the positions taken by governments related to their economic interests based upon their access to ocean resources and their technical capability to exploit the resources.

Part 2 focuses on the development of United States policy toward the conference and an evaluation of the treaty negotiations thus far. Eckert's appraisal of the two competing processes of international law making is extremely well done. The enclosure movement is the older process of the two. In fact, the rush of coastal states to assert claims similar to Truman's 1945 proclamation brought about the second process through the first Law of the Sea Conference (UNCLOS) in 1958. This conference failed to halt the enclosure movement, as did a second UNCLOS convened in 1960, and a third, UNCLOS III, which began in 1973 has not yet been successful after six years of preparatory work and another five years of meetings. In truth, these conferences have had the ironic effect of accelerating the enclosures they were designed to halt. The anticipation of an international authority beyond state jurisdiction has given states the incentive to broaden their claims prior to the creation of any such authority.

Eckert shows that a major problem is the structure of UNCLOS which influences the outcome of the negotiations, and in some cases, the lack of them. The rule of one-nation, one-vote determines what items are placed on the agenda and put to a vote. The "traditional" law-of-the-sea countries are clearly few in number.

I cannot escape the feeling, after reading this work, that the United States delegation has not confronted fundamental questions raised by Eckert, and that our policy lacks a comprehensive theoretical approach and a consistent goal. This is the best work on the subject that I have read and should be considered required reading for any student of the effort to produce a basic ocean treaty to become new international law.

CHARLES L. COCHRAN

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Beyond the North-South Stalemate. By Roger D. Hansen. (New York: McGraw-Hill, 1979. Pp. xv + 329. $12.00.)

The post-1945 international system ushered in two major changes: East-West bipolarity with its nuclear paraphernalia, and the rise of a self-assertive Third World. Those who teach international relations from a world perspective found a lot of literature to analyze the first area of "major tension," but not nearly enough to deal with the second. Writings in the dynamic political economy field have been useful, but some have also been very particularistic (either northern à la Tucker or southern à la Frank), and thus the global dimension of the North-South nexus of relations has been overlooked. Hansen's seems a welcome corrective step.

The book's organization is both logical and clear: examination of inter- and intrastate aspects of the evolution of developing and developed countries as "a necessary antecedent to the informed analysis of specific policy questions and prescriptions which remains the ultimate purpose of this book" (p. 87). Part 2 is an analysis of the international system focusing on North/South relations, dealing both with academic concepts (Ch. 2) and the protagonists' perspectives (Ch. 3). After this *what* question at the macro level of analysis, we move to the *why* question: determinants at the national level (Part 3), both in the South (Ch. 4) and the North (Ch. 5). Thus the black box is unboxed. The discussion is synthetic, informed, informative, and makes use of recent material in political analysis (e.g., corporatism). Part 4 moves smoothly then to the heart of the problem: *how* to meet this challenge in the 1980s. Hansen restricts his in-depth analysis to three ingredients that are plausible candidates to extricate us from the stalemate in the coming decade. The "integration ingredient" or scenario attempts to co-opt the upper tier of developing countries into the existing system: India, Brazil, Saudi Arabia or Nigeria. This chapter concludes with synthetic arguments and a critical evaluation of this scenario's different outcomes in different issue-areas. The global reform scenario has six variations ranging from significant trade liberalization to a holistic view of population stabilization. The last scenario, basic human needs, deals with people and not billiard-ball-like states. The author's choice? "Without the addition," he says, "of the basic human needs ingredient . . . and without Southern willingness to work with Northern states to develop this protean concept into a set of policies developing countries can support, the failure of the global reform approach may be indeed unavoidable" (p. 296).

Thus the author's perspective lends unity to the three ingredients. A continuum with minimalist/maximalist dimensions, the ingredients' flexibility is heightened because they overlap rather than being mutually exclusive. Interdependence of normative and analytical questions is indeed emphasized, through distinguishing them is sometimes like splitting hairs (p. 27), or results in occasional normative bias, such as the association between leftist and unstable regimes (p. 274).

In addition to usual misprints (e.g., pp. 27, 88, 124, 135), there are exaggerations and even inaccuracies. For instance, even though dominated by political questions, non-aligned countries did not separate politics from economics, e.g., the special Cairo Economic Conference convened as early as 1962. The book's five figures and nine tables are very well integrated in the body of the arguments, but some reveal ambiguities, e.g., what does a Middle East include/exclude: Afghanistan, Israel, North Africa, Turkey, Pakistan?

Elements of strength, however, tip the balance. The book's development is dialectical: arguments and counterarguments lead to a main cumulative thesis. Not only are excessive footnotes avoided, but also the raising of "grand" questions reward and maintain the reader's interest: e.g., the crisis of societal values, with a representative analysis of authorities' views. The discussion is interdisciplinary (e.g., law of the sea issues). Result: not only is the weakness but also the strength of the weak revealed. "The world's weakest countries. . . ," Hansen argues cogently, "coalesce in a diplomatic bargaining bloc that (1) hires Northern consulting firms to plan strategies and aggregate evidence supportive of new international policy initiatives, (2) controls agendas of most major international organisations to the dismay of great and middle range powers, and (3) links issues across organisations and agendas. . ." (p. 28).

Simultaneous publication of a paperback edition makes the study readily available as a textbook. A regrouping of abbreviations with their full title equivalents placed at the beginning of the book could facilitate use of the book for the non-specialist, and thus widen the audience of such an important subject.

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Contemporary International Relations of the Caribbean. Edited by Basil A. Ince. (St. Augustine, Trinidad/Tobago: Institute of International Relations, University of the West Indies, 1979. Pp. iv + 358. Price not given.)

The title of this book is attractive. It suggests that finally there is an attempt to view the Caribbean as a distinctive international sub-system, as a region having special characteristics and problems which separate it from the rest of the Western Hemisphere. In addition, the editor's introduction asserts that this collection of 16 essays is indeed a major step toward giving the Caribbean its own identity. The results, however, are not so encouraging. Essentially, these essays deal with the English-speaking Commonwealth Caribbean, primarily with the policies of Jamaica, Barbados and Guyana, and the result is a rather limited perspective. The book's title, therefore, should not be taken too literally.

The Caribbean region has always suffered from an identity crisis which has grown out of the peculiar mix of political, racial, linguistic, and cultural patterns there. It has been viewed as a region of United States influence, as simply an extension of Latin America, and as an area of lingering British, French, and Dutch colonialism. Efforts at integration have so far fallen short because of the centrifugal tensions and conflicting loyalties of the various states, and little progress has been made in reconciling the divergent orientations of the Latin Caribbean with those of the Commonwealth states. That the confusion over identity is likely to continue is confirmed by the publication of a collection such as this which provides so selective a view of the region.

All the essays are by authors from the Caribbean and their emphasis is almost entirely on the economic aspects of foreign policy. The essays are grouped into four sections. The first, which seeks to link the Commonwealth Caribbean with the Third World, contains the most useful essays. Vaughan Lewis, for instance, discusses the non-alignment strategy of Guyana and Jamaica as an indicator of a shift in orientation of the Caribbean states as the region becomes more active in resisting "the politics of hegemony." Locksley Edmondson and Peter Phillips assess the impact of race in determining foreign policy and, in particular, in pushing the Caribbean closer to a Third World identity. While the authors may place too much of a burden on race as a foreign policy determinant, their essay does call attention to an important and little-understood variable that has implications even beyond the Caribbean.

The growing linkages between the Commonwealth Caribbean and Latin America are detailed by Anthony T. Bryan, whose contention that membership in the OAS for Barbados and Jamaica resulted more from economic motives than from security needs or a sense of hemispheric community, raises some important questions for the future of the OAS in the Caribbean. The relationships between this sub-region, Latin America and the United States may prove to be more crucial than the African/Third World connection advocated by the other authors in this section, especially if one considers the political and security aspects of international relations as well as the economic ones. In a related essay by Angel Calderon-Cruz, the interesting prospect of Puerto Rico playing a leading role in the Caribbean is regarded as unlikely.

The second section on metropolitan ties is less successful, as the authors who deal with internal policies of Guyana and the Bahamas prescribe some rather simplistic guidelines for linking national leaders to a mass political consciousness. Part 3 concerns political processes in Trinidad-Tobago and Barbados, but tends to neglect foreign policy. Basil A. Ince's note on the "non-salience" of foreign policy issues in Trinidad is revealing, although his essay appears to lecture the Trinidad government on the need for an educated populace to enhance the "salience" of foreign policy. The essay by George L. Reid, a Barbadan official, on his country's foreign policy reflects a realism seldom found in the other articles. A fourth section on economic development and integration covers a wide range of topics and perspectives, from a radical critique of the neo-colonialism inherent in the Caribbean Common Market (Clive Thomas) to a conservative recommendation that the economies of small states can succeed if they are better managed (Courtenay N. Blackman). The essay by Jean Crusol on the "Economic Aspects of Departmentalization in the French West Indies," provides the only discussion of the French Caribbean and concludes that the effects of the close ties of Martinique and Guadeloupe with France have not been entirely positive for the two islands.

There is a void in the literature on the future of the Commonwealth Caribbean and this collection takes a small step toward filling it. Regrettably, the useful concepts and analyses are overshadowed by the book's lack of focus, the out-of-date statistics, and the uneven quality of the essays. The disjointedness of the collection is not helped by the lack of editorial coherence. Only a brief essay introduces the volume and there are no introductions to the various essays which could have served to identify common themes and to integrate the conclusions.

HAROLD MOLINEU

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Political Leadership in NATO: A Study in Multinational Diplomacy. By Robert S. Jordan with Michael W. Bloome. (Boulder, Colo.: Westview Press, 1979. Pp. xiii + 316. $20.00.)

Little is known and less written about the role of the secretary-general of NATO. This work fills a large part of the gap in our knowledge. Robert Jordan's book may also have accomplished this task too well. His conclusions about the limited political scope of the post leave little incentive for others to pursue the study further.

Once hailed as a leader of the free world—an image that at least one occupant of the post cultivated—NATO's secretary-general has since fallen on mean times. Paul-Henri Spaak's hopes for the position (and for himself) have been reduced to that of a lobbyist for Europe's defense needs, with the United States as the target of the secretary-general's discreet petitions. The symbol of change came over a decade ago when a professional diplomat, Manlio Brosio, assumed the headship of NATO's political branch. Unlike his predecessors, he did not have a solid domestic political base. Lord Ismay had access to Churchill; Spaak was the leader of Belgium's Socialists; Dirk Stikker had been Duth foreign minister. (The latest appointment of Holland's Joseph Luns, not treated in the book, may mark a return to the appointment of political figures.)

The weakness of the post stems, however, from deeper causes than domestic support or political skills. After Lord Ismay, none of the secretaries-general has come from a major NATO state. This is at once understandable and contributory to the post's weakness. The small European states need a champion, but a middleweight secretary-general has had to fight with heavyweights. For various reasons the latter are unable to assume the secretary-general's post. The United States must lead, but not monopolize the trappings of power in deference to European sensitivities; a German secretary-general might be too threatening; a Frenchman is ruled out by default; an Englishman is always possible, but then what would the weaker states call their own?

In no instance has a secretary-general been able to stand against the determined will of a major national state or its leadership. Spaak learned the hard way that he was neither a match for de Gaulle, who resisted NATO's authority, nor for the United States when the Eisenhower administration refused to allow NATO to assume increased economic responsibilities, a pet project of Spaak's. None of the other holders of the post has pressed its political limits as far as Spaak. Had they done so, nothing in Jordan's analysis suggests that they would have been any more successful than he. Lord Ismay confined himself to the creation of an international staff after the ideal of the depoliticized British civil service. Stikker tried to strike a special relationship with the United States as a means of exerting influence only to exacerbate NATO's already tender relations with Gaullist France. For his pains, he was rebuffed by de Gaulle and bypassed by Washington. The Kennedy administration also weakened what influence the secretary-general could exercise through his relationship with the SACEUR by unilaterally reshaping NATO strategy, easing out General Lauris Norstad (who had tried to play a larger political role) and replacing Norstad with General Lyman Lemnitzer, a bit of a martinet, who focused strictly on military issues, leaving others in Washington to fill the political vacuum. Brosio narrowed his responsibilities and ambitions even further, preferring to play the role of impartial interlocutor. He wished no part of highly charged political issues, like France's withdrawal or the Greek-Turkish imbroglio.

Other factors than those noted have also worked against a larger role for the secretary-general. The proliferation of Atlantic or European institutions—IMF, GATT, OECD, EEC, and the International Energy Agency—have restricted "NATO to its narrow defense responsibilities and [denied] the secretary-general a leading role in the nonmilitary affairs of the NATO member states" (p. 253). Shackled to military issues, the secretary-general must then be interested in using, not relaxing, military force. Detente politics has passed him by. Worse, "detente, rather than France, has emerged as the most persistent threat to NATO" (p. 254). The post of secretary-general threatens to fade away with detente (should it ever come), much like the smile of the Cheshire cat in *Alice in Wonderland.* Even when the secretary-general is around, much that is at play is little more than a smile anyway, though such gestures have had their useful moments. As Jordan concludes: "The secretary-general's primary contributions have been those of behind-the-scenes mediation and conciliation between states to minimize the effects of a dispute" (p. 267).

The book is written more from the perspective of public administration than of bureaucratic politics à la Richard Neustadt. In many ways the style fits the role of the secretary-general. Students of alliance politics and of bureaucratic behavior might still have expected more attention to political strategies and maneuverings. There are flashes of recognition in the book, like the contrast between Norstad and Lemnitzer or the "spirited rivalry" of the non-nuclear states for a seat on the Nuclear Planning Group, that major political stakes at issue, and these are usually won by the strong and swift. However, Jordan prefers to leave much of the veil covering NATO politics in place. In this sense he emulates the discretionary stance of the secretary-general that he has ably studied. (As a parting point, the appendix of leading NATO personages is a handy reference.)

EDWARD A. KOLODZIEJ

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Towards Professionalism in International Theory: Macrosystem Analysis. By Morton A. Kaplan. (New York: Free Press, 1979. Pp. ix + 182. $15.95.)

The stated purpose of Morton Kaplan's short volume is to encourage professional competence and cumulation in international relations theory. The principal thrust of this call for professionalism is a sharp attack on the quality of criticism in international relations theory; Kaplan labels most criticism a "dismal enterprise" that is often misguided, mistaken, and pre-professional. This is a book that should and will anger many readers not because they will disagree with the author's plea for reasoned criticism and analysis but because Kaplan is unreasonable and intemperate in his criticism of critics. For the sake of making a valid point about the underdeveloped state of the art of international relations theory, Kaplan frequently fails to heed his own advice; he wields a bludgeon where a scalpel is called for.

This book is really two books badly fitted into one. The second "book" contains three essays on systems analysis, on the systems approach to international politics, and on the pre-professional state of diplomatic history as a mode of inquiry. These essays are balanced and help to clarify and resolve many of the intellectual controversies that have characterized Kaplan's writings over the years. However, they should have preceded the longer introductory essay on "The Genteel Art of Criticism or How to Boggle Minds and Confooz a Discipline." Reading this essay on criticism—which comprises half the book—without exposure to or a refresher course on Kaplan's theoretical writings will almost surely boggle the mind and confuse the reader.

Despite unfortunate lapses of style and taste, much in this volume is valuable. Kaplan takes care to remind his readers that we live in a "real world" characterized by uncertainty and imprecision. Theories of international politics cannot be constructed to predict the idiosyncratic behavior of a given actor or the occurrence of a given event. The real world, unlike the models designed to study it, contains too many "free parameters" to render such predictions an exact science. Instead, we can and should develop theory capable of predicting modal behavior likely to occur within specific kinds of international systems, e.g., a balance-of-power system or a loose bipolar system. The modal behavior of actors will change with system changes because different systems contain different parametric conditions. The problem is to agree on what these conditions are and on how they relate to behavior. In this formulation, Kaplan is proposing that scholars should be aware of both the limits and the possibilities of theory. Hence, he refers to his work as "theory sketches" and his famous models as heuristic.

Although Kaplan believes that qualitative macro-level analysis offers the most promising approach to comprehending the forces shaping international behavior, he states that the methodology one chooses to study them with depends upon the question one poses. Whether national behavior is explained in the context of the interaction of system elements or in terms of individual actors is a function of the research task at hand. It is useful to remember also that Kaplan's methodology is essentially comparative. His familiar system models represent a methodology that is more properly called comparative historical analysis of international systems.

The long initial essay on criticism has one theme. This essay is, in a sense, a kind of autobiographical account of the intellectual journey into qualitative macro-system analysis that has marked Kaplan as one of the foremost international relations theorists of the contemporary age. It is essentially a detailed counter-criticism of Kenneth Waltz's critique of Kaplan's writings. The tone is personal, caustic, and unsettling. Waltz's critique and his contribution to macro-systems theory is, in Kaplan's mind set, a clear example of pre-professionalism. Not all scholars will agree with this characterization. If asked, many would cast their lot with Waltz. This does not, of course, invalidate the contents of Kaplan's counter-criticism, it merely indicates that the field of macro-systems analysis is wide open to debate. It is unquestionably one of the most controversial fields in international relations theory.

It is unclear whether the intense debate in the opening essay is an illustrative example of the peculiar stresses and strains associated with social scientific advancement or merely the intellectual sparring between two outstanding scholars in the field. If Kaplan succeeds in bringing some order and cumulation to the field of macro systems analysis by provoking more careful dialogue, the discipline of international relations may well lurch forward towards more professionalism and maturity.

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China, the United Nations and World Order. By Samuel S. Kim. (Princeton, N.J.: Princeton University Press, 1979. Pp. xviii + 581. $32.50, cloth; $12.50, paper.)

In November 1971, the People's Republic of China replaced its insular opponent as China's representative in the United Nations. Judging from statements about the international organization prior to joining, Beijing was considered by many to be a potential "radicalizer" of the world body—viewing international relations in terms of immutable struggle and opposing peacekeeping operations as efforts to enforce an unjust status quo. In actuality, however, the ensuing years witnessed no such dire scenarios. And Samuel S. Kim, in this exacting volume, attempts to explain why. Not only is China's participation in UN activities painstakingly plotted (through 1977) but Kim also attempts to show how these activities fall within the Maoist world view which dominated Chinese foreign policy during this period. In short, the PRC's UN policy is analyzed as a forum for China's global diplomacy, and that diplomacy is assessed in terms of Beijing's foreign policy priorities and its evolving view of world order.

Kim reveals the tension in China's global diplomacy between such contradictory impulses as the desire for a peaceful environment in which to carry out domestic social and economic development and the fear that such an environment would enervate the fighting will of the people and encourage aggressive behavior on the part of the imperialist enemy. Kim adds to the accumlating evidence that Beijing is uninterested in exporting revolution or in fighting wars of "national liberation" with its own military. He argues that China's opposition to the Soviet Union is founded less on a clash of national interests than an incompatibility of world views in which the Maoist notion of the superiority of the human will over technology elevates peasant populism to a strategic maxim. (Insofar as Kim's analysis is accurate, the technological/industrial orientation of the current Deng-Hua leadership suggests that a basic component of the Sino-Soviet dispute may disappear.) As Kim puts it: "The Maoist image of world order represents an underdog perspective . . . struggling to redefine the basic values and rules of the game. . . . By stressing the concepts of just and unjust war . . . its values are oriented toward 'world justice' rather than toward 'world order' " (p. 92).

Indeed, the belief in the justness of "revolutionary violence" places China in an anomalous position with respect to UN peacekeeping activities. Kim lucidly highlights the contradictions and ambiguities of China's positions on disarmament and UN peacekeeping arrangements in the Middle East. But, he also argues that PRC diplomats are aware of these weaknesses and have devised a way of declaring the PRC's position for the record without obstructing the will of the majority in the United Nations with which it identifies—the Third World. The device China has developed is *non-participation.* That is, Beijing registers its position in open debate and private consultation but will neither cast a negative vote nor abstain on resolutions it opposes. As Kim puts it, China displays a combination of passive opposition and passive cooperation (p. 229).

Kim devotes considerable attention to China's concern for the establishment of a New International Economic Order (NIEO). His own orientation is derived from the Galtung school of structural violence in the international system based on the belief that Third World poverty is a function of the industrial nations' affluence. Without debating the empirical veracity of that proposition, it may still be noted that while China's emphasis on self-reliance under Mao fit this version of the international political economy, the new leadership appears to be repudiating it. China's quest for international credits and technology, begun in 1978, illustrates a completely new orientation toward the international system which calls into question whether Kim's analysis of the international economic orientation of the Maoist period can be projected into the future.

Samuel Kim has written what will be the definitive study of the PRC's initial years in the United Nations, reflecting Beijing's overriding concern with opposition to the Soviet Union and support for Third World efforts to create what they hope will be a more just international regime. He has also documented conflicts between these values and the manner in which Chinese UN diplomacy has attempted to reconcile them. For all of this, we are in his debt.

SHELDON W. SIMON

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Nuclear Arms in the Third World: U.S. Policy Dilemma. By Ernest W. Lefever. (Washington, D.C.: The Brookings Institution, 1979. Pp. xii + 154. $9.95, cloth; $3.95, paper.)

This book is a *tour d'horizon* of the potentialities inherent in the proliferation of nuclear weapons throughout the world. It is most impressive in the completeness of its analysis, exploring every possibility—no matter how trivial or unorthodox—relating to the spread of nuclear arms beyond the five states with acknowledged atomic arsenals. I cannot think of another book on the subject which is so exhaustive in a theoretical sense as this one.

Ernest W. Lefever has an excellent command of the relevant literature and cites virtually all the important works on nuclear weapons use and control going back to the beginning of the debate over non-proliferation in the early 1960s. In addition, he provides current information on the atomic energy programs of each of the nine states investigated, as well as the costs of the various nuclear capacities that a state might develop.

The book is divided into eight parts. The first chapter lays out the theoretical framework; chapters 2 through 6 are case studies of nine potential nuclear-weapons states (India, Pakistan, Iran, Egypt, Israel, Taiwan, South Korea, Argentina, and Brazil); and chapters 7 and 8 discuss the implications for American foreign policy, particularly for its future non-proliferation policy.

Among the new theories developed is a fivefold distinction among new nuclear-weapons states as: threshold states, token nuclear force states, and states with one of three different levels of "military significance" (pp. 10–11). (The implication is that each type requires a different form of American response.) There is also an "incentive index" (p. 20) to measure the likelihood (high, medium, or low) of a state's going nuclear; this index is based upon a combination of two types of domestic political pressures, five external factors, and four assumptions about trends in international politics.

With similar theoretical classification, the level of a nation's potential nuclear capability is defined to include: minimum deterrence, massive retaliation, flexible response, denial of (opponent's) war-winning ability, and achievement of (one's own) war-winning ability. The precise level and configuration of a planned nuclear force are said to be a function of its "purpose, available resources, and time" (p. 121). With respect to actual use of nuclear weapons (an option from which Lefever does not shrink), they can be employed "to conquer, deter, defend, provoke, or blackmail" (p. 118). Finally, future U.S. non-proliferation policy includes the following options: security guarantees, promotion of international agreements, denial of nuclear technology for explosions (but never for "peaceful" power use), reward and punishment, and encouraging nuclear abstinence (pp. 122–35).

As one might suspect, there is a method behind this madness of categories, and the high level of theoretical abstraction does not mean that this book is free from unexamined ideological assumptions and personal biases. Lefever, the director of the Ethics and Public Policy Center of Georgetown University, definitely has policy preferences and his elegant conceptual framework has been erected with an eye to buttressing a value-laden political argument.

Lefever has no fear of living in a nuclear-proliferated world. He does not believe the spread of nuclear technology to Third World countries will inevitably be followed by the production of nuclear weapons. The costs and risks of going nuclear are so high that he thinks few of the nine states studied will be sufficiently motivated to make the effort; even if one did, Lefever doubts that it should threaten "strategic stability" (p. 147). He is quite confident that the U.S. and USSR have sufficient "discipline and mechanisms" in place to avoid being "provoked into accidental or deliberate nuclear responses" in, for example, the Middle East (p. 120). In short, he does not subscribe to the view that an increase in the number of nuclear-weapons states means an increase in the probability of nuclear war.

This cavalier attitude is particularly troubling when one looks more closely at some of the states included, and not included, in this book. The most glaring omissions are South Africa and Australia, two states which Lefever does not consider worthy of analysis because, one suspects, he considers them safe. The South African case is described as "problematical" on p. 22, and nothing more is heard about it for the rest of the book; Australia is not mentioned at all.

With respect to states covered, one detects a particular sympathy to the nuclear weapons needs of Taiwan, South Korea, and (pre-revolutionary) Iran. The first two are said to be faced with "implacable adversaries" (p. 19); in fact, they are twice reported to be "under seige" (pp. 22, 82). Taiwan's adversary is none other than Red (sic) China (p. 85); and South Korea's anxiety has been aggravated, in a literally far-fetched example, by "the imposition of a Marxist regime in Angola" (p. 90). Pre-revolutionary Iran was looked upon with sympathy because it was, after all, the "Guardian of the (Persian) Gulf" and led by a "dynamic" and "moderate" Shah interested only in modernizing his country (pp. 47, 63).

In short, for all its theoretical completeness, this book is much too defensive of past American positions, particularly of the "non-proliferation" policies of recent Republican administrations. The U.S. is portrayed as having done its best to restrain the spread of nuclear weapons among the potentially irresponsible. Actions such as aggressive advocacy of atomic technology exports under Eisenhower in the fifties and Nixon in the seventies are not seen as having caused the dilemma the world now faces with respect to nuclear weapons proliferation. Policies such as the Kennedy administration's multilateral NATO force and the Johnson administration's limited concessions to the Third World in the Non-Proliferation Treaty, are treated similarly uncritically.

As for today, with the American security guarantee for Third World states deemed to be dubious under President Carter, Lefever sees an added incentive to stop worrying and start learning to live with having the bomb in the hands of assorted Asians, Africans, and Latin Americans. The construction of his intricate theoretical classifications will presumably allow the reader to sleep a little more soundly secure in the knowledge that the spread of nuclear weapons around the world is not one undifferentiated disaster, but something that can be understood and appreciated for all its manifold categories.

MICHAEL J. SULLIVAN III

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The Arab-African Connection: Political and Economic Realities. By Victor T. Le Vine and Timothy W. Luke. (Boulder, Colo.: Westview Press, 1979. Pp. xvii + 155. $20.00.)

This latest book by Victor T. Le Vine, whose previous works have focused on West Africa, and by Timothy W. Luke, is a well-researched piece of scholarship. It illuminates one aspect of the worldwide energy crisis, namely, the effects on poor African states and the Arab response. Since 1972 and 1973, black African states have for the most part followed Arab states at practically every turn in the road at the United Nations and elsewhere: they broke relations with Israel after years of often mutually very beneficial relations; they supported the Zionism is racism resolution; and their voices swelled the Third World and East European chorus damning Western perfidy. At the same time, they have suffered from the rise in oil prices more than many other countries. How have the Arabs helped and what does the future look like?

In their first chapter Le Vine and Luke review briefly the peripaties of Arab-black African relations from 1945 to 1973. The ties were ambiguous at best for many reasons, but after 1967 a drawing-together began, and on the eve of the Yom Kippur War several states had already broken relations with Israel. The war itself and the precipitous rise in oil prices completed the rupture except for four states. The authors say the reasons for the change were a desire to demonstrate Third-World solidarity, the threat of oil cutoffs, (unspecified) personal threats to some African leaders, and the African hope that once the dust settled they would be granted a special low price for oil. Their hopes have not materialized, and the long-range consequences are grave for the poorest states despite some Arab willingness to expand aid programs. The problem, they say, is that although some of the very poor states—producers of coffee, for example—benefited from a concomitant increase in prices for their products, all nonetheless suffered from a doubling of their oil bills as well as from an increase in prices for manufactured goods imported from Western Europe and America.

Le Vine and Luke then deal with the response of international aid institutions; OECD countries, for example, were unable or unwilling to fill all the gaps caused by the oil crisis. Some OPEC countries such as Saudi Arabia and Kuwait began to expand generous but uneven aid programs: OPEC aid increased from $1.5 billion in 1973 to $9 billion in 1975, but most went to poor Arab or Muslim countries. In any case, "For the African countries Arab aid in all forms has covered less than 30 percent of the additional financing requirements attributable directly or indirectly to the OPEC price revolution" (p. 26). Useful tables show where Arabs and Iranians have put much of their spare cash (13 percent in U.S. Treasury bonds and notes, for example, p. 28).

The authors explain the Arab response to accusations that they have ignored the plight of their neighbors. They say that inflation in the Western countries is the culprit, but the African states are, in the view of Le Vine and Luke, disillusioned with the Arab connection. They do not, however, explain what the Africans have done to show their displeasure except to say that trade with Israel is on the increase. Some Africans still hope for a better deal from countries such as Saudi Arabia, Nigeria and Venezuela, it seems.

As for the long run, Le Vine and Luke are not sanguine. The poor African states are emerging as a new "Fourth World" just as dependent on the OPEC countries as they have been on the industrialized West. Furthermore, Africa is increasingly irrelevant in world politics. Maybe, they conclude, the First World will find a solution for themselves and the emerging Fourth World.

This book abounds with data from banks, petroleum intelligence reports, economic digests and the like. Six out of seven of the appendices—excerpts from Nasser, Senghor, etc.—are unnecessary, but the last, "Arab Financial Institutions Providing Assistance for Economic Development" is as useful as the many tables. The authors have used very few Arab sources, but they have nonetheless gleaned a great deal of information from American, British and French sources. Lastly, despite the fact that we can understand that the increased costs of publishing force a price hike for books, $20 for a volume of 112 pages (not counting the appendices) is difficult to forgive.

BRIAN WEINSTEIN

Howard University

Bear and Foxes: The International Relations of the East European States, 1965–1969. By Ronald Haly Linden. (New York: Columbia University Press, 1979. Pp. vii + 328. $17.50.)

Ronald Linden's *Bear and Foxes*, which examines East European foreign policies during the eventful last half of the 1960s, provides a significant and welcome addition to the small number of studies successfully applying behavioral methods and theories to the analysis of communist foreign policy. The bulk of Linden's analysis is devoted to measuring the actual variations among the eight East European countries along two dimensions: (1) their interactions with 12 international actors and (2) their publicly expressed attitude, if any, on 46 events concerning the Arab-Israeli War, China, Czechoslovakia, the world communist movement, other events in Europe, and the United States. The East European states are then ranked on both dimensions according to the sum of their actual deviations from the Soviet bloc mean score or norm and, more importantly, according to whether their deviance is "positive" or "negative" in terms of imputed bloc positions.

Five potential explanatory factors—level of economic development, military vulnerability to the USSR, trade dependence on the USSR, degree of political liberalization, and ethnic fractionalization—are tested for their predictive power over the degree of positive or negative deviation. The first two are significantly related to interaction but not attitudinal deviations; the third and fourth are not associated with either; and only ethnic fractionalization exerts a significant impact on both. From these findings, Linden's principal conclusions are that: (1) the need to build legitimacy in fractionalized societies is a major stimulus of "deviant" foreign policies, (2) interaction deviance also results from the economic needs of the less-developed East European states regardless of their foreign policy attitudes, and (3) overall Soviet economic leverage is fairly marginal in promoting foreign policy conformity, although a subordinate state's energy situation seems more important. More broadly, these conclusions are linked together within the framework of East's thesis that foreign policy behavior devolves from a state's "capacity to act" which, in turn, should be influenced by its levels of development and societal stress. The results of the aggregate data analysis receive further support from a nonquantitative case study of Romania which suggests two further "enabling factors" for independent foreign policies—a unified indigenous leadership and a favorable international environment (e.g., the Sino-Soviet split).

Linden's analysis is also stimulating in that it suggests broader research endeavors. For example, an alternative interpretation of the statistical relationship that emerged between development level and interaction deviance can be explained by other factors adumbrated by Linden to indicate another area for further scholarly burrowing. One might argue that the greater interaction deviance of the less-developed East European states stems from their economic opportunities rather than from their economic needs (i.e., industrialization increases Soviet-East European complementarity) and that the greater deviance of the less-developed Albania, Yugoslavia, and, to a lesser but still significant extent Romania, initially derived from political, not economic, factors. Linden's own analysis indicates two such political factors—leadership unity and regime legitimacy needs as conditioned by societal stress. More broadly, a regime's decision to base its power on internal or external sources of legitimacy and support seems crucial to its foreign policy strategy. The explication of such strategies in terms of both internal and external factors (e.g., party strength, ethnic conflicts, Soviet pressure, etc.), then, forms a major research task that flows from Linden's conclusions. Such a task would obviously require the integration of causal theory, comparative aggregate data analysis, and traditional research about diplomatic history and "Kremlinological" intrigues.

Perhaps the major criticism which can be directed at this book is that the theoretical framework linking the myriad tables and quotations into a coherent whole is not made as explicit nor as comprehensive as it might have been. The introduction sketches a theory of "normative integration"; yet, except for a brief but interesting discussion of bloc cohesion in the conclusion, this theoretical theme is almost totally ignored in the text. Rather, as the metaphor in the title indicates, Linden is primarily interested in the relative degrees of deviance and autonomy in the foreign policies of the "foxes." Thus a more comprehensive theoretical structure, introduced before the data analysis, would have been highly desirable to direct the reader in addressing this question.

Bear and Foxes, in sum, is an excellent piece of behavioral research. Linden develops sophisticated measures of foreign policy orientations (the best available for Eastern Europe) and systematically tests propositions or theories about why these foreign policies differ. The research can also make a cumulative contribution to our understanding of Eastern Europe since it suggests broader interpretations and avenues for analysis. This book should be read, and the questions that it raises should be attacked.

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International Economic Sanctions: The Cases of Cuba, Israel, and Rhodesia. By Donald L. Losman. (Albuquerque: University of New Mexico Press, 1979. Pp. xi + 156. $12.95.)

Under what conditions can international economic boycotts/embargoes prove politically successful, either by forcing the target state to be politically compliant or by galvanizing a change in the target state's leadership? This is the question that guides Donald Losman's welcome investigation into three contemporary cases of economic sanctions.

An early theoretical chapter on the diverse gains to trading nations sets the stage for his excellent and detailed review of the costs incurred by Cuba, Israel, and Rhodesia as trade was denied them over the past 15 to 30 years. Based on thorough research, the nuances of the direct and indirect costs of sanctions for the target economies are carefully traced and soberly weighed. However, Losman necessarily must often rely upon educated guesses and, particularly in the case of certain "foregone potential" costs, he confesses that sometimes there is simply no basis for reasonable estimates. Nevertheless, he can roughly weigh and sum the very substantial costs for the three target economies in question.

Why, despite their economic effectiveness, has each of these cases of economic sanctions met with political failure? Broadly speaking, there are two ways in which one might approach an answer. On the one hand, the author concludes (*post hoc*) that economic sanctions must satisfy six conditions if they are to be politically successful, conditions not met in the cases at hand. Alternatively, however, one might begin by challenging Losman's regrettably static and dichotomous definition of political success as a change of policy or government in the target state. Just as the initiation and maintenance of economic sanctions are presumed to have predictable dynamics, might not political success also be more properly regarded as a process? For example, as the United States led the sanctions against Cuba, among its stated political objectives were to reduce Castro's capacity "to export subversion and violence" in the hemisphere and to demonstrate that "communism has no future" in the Americas. Put differently, an alternative definition emphasizing political processes would incorporate some less dramatic, less immediate, and less tangible successes, potential gains that might in fact be achieved even if the author's six conditions for "success" were not fully satisfied.

One candidate vehicle for moving this study farther down the road to political theory is already available in the literature on asymmetric international interdependence. From this perspective, the book's conclusions make two important points. First, the three cases support the contention that the immediate costs of economic sanctions are insufficient conditions to imply policy change by the target state. To coerce a shift in policy also requires that the target state be unable to respond in ways that minimize the longer-term costs. Losman's more significant contribution is then to draw attention to the internal distribution of costs as political determinants of the state's capacity to withstand sanctions and tenaciously stay its policy course. It is precisely such disaggregations of the national political and economic unit that many students of international interdependence may find to be particularly worth pondering.

In several respects this book compares favorably to Margaret Doxey's treatment of the subject (*Economic Sanctions and International Enforcement,* Oxford, 1971). For example, Losman's determined adherence to the guiding trade theory results in a more systematic pursuit of evidence. Additionally, and although Doxey has foreshadowed several of Losman's more interesting observations, Doxey emphasizes the organizational form of collective sanctions, whereas Losman introduces collusion (oligopoly) theory in order to stress the process. Specialists in international political economy should therefore appreciate the contributions of this solid and insightful study, even as they must continue on their own to develop theories of international political processes.

NEIL R. RICHARDSON

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The Angolan Revolution: Vol. 2, Exile Politics and Guerrilla Warfare (1962–1976). By John A. Marcum. (Cambridge, Mass.: MIT Press, 1978. Pp. xiv + 473. $25.00.)

With the publication of the second volume of his study of the Angolan Revolution, John Marcum has added one of the finest contributions to the literature on African nationalism—a field which has in the last 20 years attracted an august group of social scientists. Marcum was faced with a particularly difficult task, not encountered by most earlier researchers; Angola went through a long violent independence struggle without achieving a united, cohesive and coherent nationalist movement, and American scholars could no longer tap the reservoir of good will and trust which had existed for them among nationalist leaders in the 1950s and early 1960s. The dizzying detail which this research has produced is therefore all the more admirable. Indeed, it is clear that Marcum is committed to the proposition that one must exhaust historical research in order to understand how societies like Angola evolve. He respects detail and is hesitant in applying recent methodological innovations or making the subject of his inquiry an "illustration" of a theory. Since the material he deals with has not been properly put together and recorded before, this too adds to the value of the study.

The Angolan Revolution, Vol. 2, covers the period from 1962 to 1976. In other words, it deals with the independence struggle from the time when violent confrontation with the Portuguese began to the civil war which surrounded the achievement of independence. Apart from following the chronological evolution of events, the book is structured around the development of "two-party" and later "three-party insurgency." The parties concerned were the victorious MPLA and the two losers, the FNLA which had been in the picture from the beginning and UNITA which had emerged in 1966. Needless to say, there were numerous secondary organizations also involved and the author analyzes them with punctilious care. Marcum contrasts the three main movements on the basis of the following variables: ethnicity, religion, class, culture, and race, and shows that there was a certain socio-political similarity between the FNLA and UNITA while the MPLA was in many ways distinctive. The FNLA and UNITA were both ethnocentric, anti-*mestico,* populist and rural, while the MPLA was multiracial, urban and led by acculturated intellectuals. There is some suggestion that these differences are somehow related to the ultimate result of MPLA victory, but to me a far more persuasive reason emerges in the detailed analysis of repeated leadership failure, especially in the FNLA. In his conclusion Marcum points to the FNLA's "structural incoherence ... while Roberto [Holden, the president] continued to run the movement as a one-man show devoid of regular process or cohesive organization" (pp. 275–76). Of course, social scientists have found leadership performance a difficult matter to assess. It is once again to Marcum's credit that he does not avoid this topic—central to the evolution of the Angolan independence struggle, as it turns out—despite its many pitfalls.

The Angolan nationalist movement spent most of its energies in fratricidal quarrels and in fact appears to have engaged the Portuguese relatively little. During the largest portion of the time covered by this volume, the leaders, and many of their followers, were out of Angola, and suffered what the author calls "the dysfunctional impact of exile" (p. 181). In effect, that meant that they were endlessly dependent on others: the leaders of host countries, international relief agencies, the Organization of African Unity, supplies from competing major and minor powers, etc. Under these circumstances "revolution" often amounted to little more than good public relations with foreign benefactors. "Title and status ... seemed within reach of anyone with imagination enough to invent a new Angolan organization" (p. 181). There were of course some leaders who tried to break out of the stalemate of inter-party exile politics and—consequent?—Portuguese success in maintaining power. UNITA was in fact founded for this very reason. Yet "the absence of appreciable external support" (p. 221) devastatingly limited UNITA's insurgent capacity, and it received no serious support (until very late in the game) because the two earlier movements had in effect tied up potential supplies.

Despite the sympathy with which Marcum describes Angolan nationalism, it is difficult to close this book without feeling that this was in reality a rather unsuccessful revolution. The colonial enemy collapsed because of a military coup/revolution in the metropole. The competition among nationalist parties was decided by massive foreign intervention. Comparing the liberation movements of Portuguese Africa with other, better organized, movements, Marcum argues that despite their weaknesses "they refused to give up ... [and] over time ... developed into potent catalysts of social economic, and finally, political change" (p. 179). It is certainly true that the colonial wars were the immediate causes for the Portuguese coup/

revolution, but it would have been more interesting and germane to emphasize extensive comparison of the Angolan nationalist movement with those of Mozambique and Guinea-Bissau. It is difficult to imagine that in such a comparison Angola would have emerged favorably. Moreover, the price of this internal weakness has been great; it caused major power intrusion in Africa, a bloody civil war *after* the victory of the independence struggle, continued internal strife, and finally a regime heavily dependent on foreign military support for its survival. None of these characteristics can be found in either of the other two Portuguese ex-colonies.

Finally, it is important to point out that this description of the Angolan Revolution is in some respects incomplete. For instance, there is little analysis of mass mobilization by any of the parties. How were units organized? Who were the local political and military elites? Did the deep cleavages in ideologies affect recruitment and organization in the rural—and ethnic—home bases of the main parties? It does not in any way detract from the value of this study to say that it is essentially an institutional history and analysis of Angolan nationalism with emphasis on the political elite and its external and inter-party relations. In this respect it is a brilliant, painstakingly worked, and intellectually honest study which no student of Africa or nationalism can afford to ignore. Indeed, Marcum sets a standard of analysis and historical reconstruction which will be hard to emulate.

HERBERT F. WEISS

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East Germany and the Warsaw Alliance: The Politics of Detente. By N. Edwina Moreton. (Boulder, Colo.: Westview Press, 1979. Pp. xv + 267. $18.00.)

American students of European foreign relations are accustomed to viewing the German Democratic Republic through the eyes of a few distinguished West German scholars. While noted for their comprehensive analyses and frequent insights, these observers are singularly unsympathetic to the aspirations and limitations of the East German government. Consequently, we have acquired an image of East German foreign policy as erratic, petulant, and generally counterproductive, especially with respect to Berlin and "the German problem." We perceive pointless harassment of Berlin travelers, vindictive incitement of the Warsaw Pact invasion of Czechoslovakia, an arrogant Walter Ulbricht justly deposed at Soviet direction in 1971, followed by Erich Honecker's unprincipled subservience to Moscow's every whim.

Edwina Moreton's concise treatise directly confronts these and other one-sided perceptions. The result is by no means an apologia for the East German regime, but rather a painstaking attempt to weigh the sometimes contradictory evidence surrounding the GDR's conduct of foreign relations. Eschewing the biases of Bonn and Washington, Moreton provides a carefully reasoned account of East Germany's relations with the Federal Republic, the Soviet Union, and the rest of the Warsaw Alliance, chiefly in the period 1967–1971.

The book begins with a review of postwar German developments, highlighting the points of interpretive controversy. For example, Moreton asserts that, in Soviet eyes, the issue of Germany's future was separate from the questions concerning the rest of East Europe, which acquired communist regimes more or less according to Soviet design. As for Germany itself, Moreton asserts, "It would probably be an overestimation to assume that Stalin had a policy at all." Thus, "it was ultimately a Western decision to cut Germany in two" (pp. 13–14).

In fact, Soviet inability or unwillingness to specify priorities in its European policy has continually subjected the Moscow-East Berlin partnership to severe strains. During tis second decade the GDR found its identity still in limbo, as Soviet and American leaders entertained proposals to resolve German issues without enhancing East Germany's problematic sovereignty. Ironically, it was Adenauer's "intractability" which moved Moscow toward firmer support for its client.

The Berlin Wall and economic reform brought a sense of self-assurance to East Berlin. And beginning in 1967 the latent differences between East German and Soviet goals became manifest. Moreton traces GDR (and Polish) disagreements with the Soviet Union over appropriate responses to Romanian and Czechoslovak developments in the late sixties, while discounting any decisive role for Ulbricht in the decision for military intervention. Subsequent East German resentment over Poland's response to Brandt's new *Ostpolitik* is portrayed with solid evidence.

The status of Berlin and the control of transit routes has been an extremely sensitive and symbolic concern to East German leaders. Time after time, Soviet failure to unreservedly support the East German position on Berlin produced ill feelings. Moreton's documentation of this crucial issue is meticulous, almost to the point of being repetitive.

The author argues persuasively that Ulbricht's 1971 retirement was welcomed, but not engineered, by Moscow. In her view, there was ample cause for dissatisfaction with the party leader among his domestic cohorts. Moreover, the substantive disagreements between the two allies persist under Honecker. On the other hand, more frequent consultations have now become the rule, producing more consistent policy and less frequent GDR recourse to disruptive tactics.

The major East-West treaties of 1970–71 are judged mutually beneficial, but favoring Bonn somewhat more than East Berlin. Moreton also notes approvingly Honecker's willingness to abandon Ulbricht's "illogical" attachment to the concept of a single German nationality. Unfortunately, she makes no attempt to explain the rationale for Ulbricht's preference.

This book has immense value as the only substantive and balanced treatment of East German foreign policy to appear in either German or English. The topics covered may be familiar. But no reader will fail to notice the contrast between the subjectivity of earlier work and the careful scholarship of this truly fine study.

C. BRADLEY SCHARF

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Why War? Ideology, Theory, and History. By Keith L. Nelson and Spencer C. Olin, Jr. (Berkeley: University of California Press, 1979. Pp. 201. $10.95.)

I used *Why War?* in my graduate seminar on Conflict and Peace, partially as a companion to Kenneth Waltz's *Man, the State, and War,* but moreover as an exercise in writing reviews as professional training. Since the perspectives offered by these students differ somewhat from mine, I decided to include excerpts from a few of them.

Steve Toth comments:

> The authors' purpose in writing this book is to look at the "general tendency" in which men of the historical profession have studied history in general and the cause of war in particular. Nelson and Olin concern themselves with the different manners and approaches and the many groupings and broad theories that are espoused in that profession. They expore these major elements in adequate form. . . . They believe that theories come from the way in which their authors perceive things, i.e., their ideology. This is noted in order to point out the effects of subjective factors in theory-building so that better means can be developed to test the "usefulness" of theories. . . . Nelson and Olin have taken a page out of the Political Science field and transported it into the field of History by changing the names of the concepts widely used by political scientists. Instead of using categories such as "idealist" and "realist," Nelson and Olin created "new" categories of "conservatism," "liberalism," and "radicalism."

Carolyn Shahan comments:

> Nelson and Olin also tend to throw aside too lightly the aspect of complexity. . . (pp. 1–2). If historians have stressed complexity in order not to . . . deal with explanations of events, or not to develop theory, then this is a legitimate criticism. However . . . complexity should not be excluded from consideration or we will be robbed in another way of comprehending the world in which we live. . . . Galtung has proposed a rich theory, parts of which have been tested and substantiated by empirical data. Nelson and Olin's classification of Galtung in the radical theory discussion because of structure and imperialism seems too simplistic, and possibly does a great deal toward misrepresenting Galtung's work. . . . There seems to be too much of an effort by Nelson and Olin in selecting the authors which will be discussed as those that "fit" the theoretical categories (p. 141, for example) whether they actually fit the categories. . . . Another weakness in Nelson and Olin's cursory presentation of the three ideologies and their application in analysis is that the three categories are not mutually exclusive or clearcut. Two examples of this are on pp. 153–55 during the discussion of Taylor as an example of the liberal tradition, but at the same time pointing out various aspects of Taylor that appear to be conservative and radical, and on p. 182 during the discussion of Hildebrand as an example of the radical tradition, but identifying elements of liberal group conflict theory in Hildebrand's historical interpretation. Thus, a problem seems to exist in Nelson and Olin's work with . . . internal consistency.

Leonard Zalewski comments:

> The title *Why War?* leads one to imagine that we are about to read a scholarly discussion on why nations and people engage in mortal conflict. This is not at all the case. What Nelson and Olin have accomplished is the composition of an interesting review of historians who have, in fact, looked at wars through the narrow tube of a telescope in order to justify their particular ideology. . . . [Conservatives] are the writers who resist such admirable ideas as individualism, notions of equality, faith in reason and science, and belief in progress. It may become apparent to the careful reader that the conservatives are the ones that the authors identify as

> being opponents to the good that is being introduced by the liberals, and that they will even resort to war to take the people's mind off their internal domestic difficulties.... The naivete of the authors is apparent, when they conclude that a lack of interest in ideology and theory prevents historians from making hypotheses regarding the causes of war that explain them satisfactorily.

I think that the basic idea of *Why War?*, that ideology influences one's interpretation of history, is sound and generally well presented. However, I feel that my students accurately perceived a number of weaknesses: Procrustean categorization, non-acknowledgement of political science concepts and theory, over emphasis of the role of ideology, and a lack of in-depth analysis, especially in the last two chapters on World Wars I and II. To the problem noted by Shahan in this regard, I would add that the selection of the two world wars biases the outcome. Nelson and Olin choose these two wars "because they are obviously important, complex, and well-documented" (p. 92). These wars were and are also highly emotional and ideologically laden. No wonder the authors find ideology coloring histories of these wars. I doubt they would have the same level of "success" in analyzing scholarly writings on the Seven Weeks' War or the Crimean War.

The non-use of political science concepts and the lack of an in-depth analysis go hand in hand to some extent. For example, although "levels of analysis" is acknowledged in passing in a couple of places, Nelson and Olin do not really explore the interplay between levels and ideology. A very powerful set of analytic categories can be created by crossing the ideologies with Waltz's images, as shown in the matrix below.

Such a matrix helps locate the Fischer-Ritter debate (p. 117 ff.) more precisely. Nelson and Olin place them both in the liberal box, raising an anomaly. In the matrix, though, Fischer would be Liberal-State Structure with clear tendencies toward Radical-System and Conservative-Individual. Likewise, Ritter starts in the Liberal-State Structure box but is pulled in the opposite direction, toward Radical-Individual and Conservative-System. Thus, they are only similar in the singular concern with the German bureaucracy and interest groups; otherwise they remain opposites.

Furthermore, because the matrix does differentiate between levels as well as ideologies (philosophical predilection), one can raise some further issues. For example, liberals generally place the blame for war on the state or international system structure, not necessarily because they don't believe in government intervention (Nelson's and Olin's claim), but because they believe the individual is good; since there are wars, it must be the society or the international system that is bad. The conservative/pessimist, who believes man is fundamentally flawed, can be found at all levels, since man's creations reflect his basic failings.

Lastly, the categorization of historians (or political scientists, or any other group) may help us to understand their work from an analytic point of view, but does it really help us answer the question, why war? I think not.

CYNTHIA CANNIZZO

Ohio State University

Arms and Politics, 1958–1978: Arms Control in a Changing Political Context. By Robin Ranger. (New York: New York University Press, 1979. Pp. viii + 280. $19.95.)

This volume surveys U.S.-Soviet negotiations on arms control during the past two decades. Robin Ranger begins with a somewhat abbreviated and selective discussion of arms control thinking in the United States during the late 1950s, emphasizing what he sees as its heavy technical focus and contrasting such an approach with the more political one taken by the Soviet Union.

The book provides a useful summary of the negotiations during the period but at times tends to be somewhat repetitive as in the

	Ideology		
	Conservative (Pessimism)	Liberal (Optimist I)	Radical (Optimist II)
Individual (Human Nature)			
State Structure (Groups)			
International System			

scattered discussion of MLF and Soviet concern over German access to nuclear weapons. The author has a properly pessimistic view of the ability of the Nonproliferation Treaty to stop proliferation and indeed seems almost to question whether nonproliferation is an appropriate goal for the United States. The NPT, to the extent that it works, is seen as benefiting the USSR, for the potential proliferators are more likely to identify with the United States than with the Soviet Union.

Although Ranger views Kissinger and Nixon as taking a more realistic position on the political aspects of SALT by recognizing its utility in furthering detente, he is critical of the agreements reached, viewing them to be primarily to the advantage of the Soviet Union. He is particularly critical of President Carter's nuclear policy on SALT and non-proliferation, suggesting that it represents a return to technical arms control. Since the study ends prior to the conclusion of the SALT II Treaty, the author is unable to evaluate that treaty but would clearly side with SALT opponents such as Paul Nitze and Senator Henry M. Jackson whom he labels as revisionist arms controllers.

The basic thesis of the book is that the United States' efforts to deal with strategic arms control from a technical perspective have failed, whereas the Soviet Union's emphasis upon political arms control has succeeded. The dichotomy which Ranger makes between technical and political approaches to arms control is drawn far too rigidly. The United States government has not been as naive as the author sometimes suggests, for the political implications of the various agreements have been very much in the minds of U.S. decision makers. It may even be that technical arms control proposals are made for the express purpose of assuring their defeat, as in the case of President Nixon's decision to insist upon including on-site inspection in U.S. proposals for MIRV restrictions during the SALT I negotiations. One might also question why the author always reads underlying political motivations into Soviet statements and proposals that on the surface would seem to meet his definition of technical arms control while viewing all U.S. proposals at face value.

The volume takes a decidedly hard-line position vis-à-vis the Soviet Union as illustrated by the assertion that the Soviet Union "violated" the unilateral moratorium on testing in 1961. If it was a unilateral moratorium, I fail to see that there is any violation involved. This basic hard-line attitude can also be seen in the author's reaction to the Soviet concession of removing the issue of Forward Based Systems from the SALT negotiations. This concession is used as evidence for his assertion that the Soviet Union's concern about these systems was only a bogus issue since it gave them up so easily. Nor can I agree with Ranger that the SALT agreements have been decidedly to the advantage of the Soviet Union, for they allow both sides to produce virtually every weapon system they desire.

Despite these reservations, I find the book a useful summary of the disarmament negotiations during this period. Particularly useful for the American reader is the book's continuing emphasis upon the implications of these various negotiations for European security interests.

LLOYD JENSEN

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Beyond Functionalism: Attitudes toward International Organization in Norway and the United States. By Robert E. Riggs and I. Jostein Mykletun. (Minneapolis: University of Minnesota Press, 1979. Pp. 224. $19.50.)

The functionalist approach to world order has beguiled scholars since the publication of David Mitrany's *A Working Peace System* in 1943. A key functionalist assumption is that problem-by-problem cooperation across national boundaries induces greater receptivity to further cooperation and enhances "community-mindedness." This study addresses that assumption empirically. It looks at attitude change resulting from experience as participants in international organizations, or in dealing with them, among officials and national legislators in Norway and the United States. The UN General Assembly is the main terrain for participation. The main, but not only, method used is "static group comparison," which compares attitudes of respondents who have been participants with attitudes of control group members who have not. The primary focus is on "affective" changes, that is, changes in feelings or beliefs, but there is also some attention to "cognitive" changes, or changes in understanding. The effects of selected background characteristics of the respondents are also examined.

The work is lucid and intelligible, even to readers who may not be steeped in the techniques employed. It is technically solid. The logic is always careful. Both the design and the interpretation of the data are sensitive to the inescapable ambiguities of attitudinal research, among them the possibly biasing influence of self-selection among participants. Although the book is relatively brief, it represents a substantial, well-documented research effort, and considerable resourcefulness. The main sources of data were questionnaire inquiries, keyed to the nine hypotheses (p. 38). In addition, however, interviews and content analyses were used. The research design and results are set out in four main chapters, preceded by a brief synopsis of the functionalist idea and literature, and followed by a concluding essay bearing the title of the book which, admittedly, moves well beyond the data presented earlier to "more speculative, intuitive and subjective" conclusions (p. 160). There is, alas, no index.

Unhappily, the authors do not explain why Norway was chosen as the country to be paired with the United States. The choice is influential, given Norway's ranking well above the United States and ahead of most other countries in Robert Angell's "Index of National Support for World Order" (in *The Quest for World Order* [Michigan, 1979], p. 19), and evidence of Norway's strong disposition toward global cooperation in Hveem's 1972 study (*International Relations and World Images: A Study of Norwegian Foreign Policy Elites,* cited in Angell, pp. 45–46). As expected, the results show the Norwegians to be generally more disposed toward the UN and working through it than the Americans are. This basic difference does not, however, seem seriously to affect the influences of experience on attitudes.

Another difficulty is the authors' choice of the UN General Assembly as the surrogate for the functional arenas of functionalist doctrine. The authors deal with the dilemma that functionalism posits progress by way of cooperation on issues of low political controversiality, whereas the General Assembly is a very political body, by arguing that "a finding of predicted change would be all the more convincing" (p. 31). That is not altogether persuasive, however, because it does not allow for the possibility of negative or ambiguous conclusions shaped by the forum in which attitudes were formed. That possibility seems to have been realized in some of the conclusions of this study.

The central conclusions are interesting all the same. Overall, experience does promote more favorable general evaluations of the UN. For the Norwegians, that carries over into more affirmative views of action commitments—strengthen the UN, be more involved in it, increase its financial resources, channel more foreign aid through it—while the reverse is true for the Americans. For both groups, experience strengthens affirmative outlooks toward more general forms of international cooperation—give trade preferences to developing countries and promote economic development of poorer countries. In neither case does experience correlate with enthusiasm for giving the UN greater authority to enforce rules, which the study uses as an indicator of willingness to transfer sovereignty to international organizations. Finally, among the Americans, the effect of experience is greater for members of Congress than for officials. Altogether, "the relationship between participation and positive affect is complex and conditional" (p. 144).

Although resorting to surrogates for data more directly linked to functionalist behavior inescapably introduces ambiguities affecting the authors' main purpose of testing an important functionalist hypothesis, the book is nevertheless rich in nuanced analysis. It certainly makes a substantial contribution to the literature on attitude change.

LAWRENCE S. FINKELSTEIN

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Plutonium, Power, and Politics: International Arrangements for the Disposition of Spent Nuclear Fuel. By Gene I. Rochlin. (Berkeley: University of California Press, 1979. Pp. xiii + 397. $22.95.)

Gene Rochlin's focus is the search for reasonably safe solutions—technological, economic, political—for management of the "back end" of the nuclear fuel cycle. He deals primarily, though not exclusively, with the United States. It is impossible to compose a brief summary of his arguments. The fuel cycle from beginning to final disposition of residues is previewed in an admirable diagram (p. 12). The text is divided into groups of chapters focused on "policy framework," "technical" and "political settings," and "prospects for international fuel cycle arrangement." The concluding chapter pulls the variables together in a synthesis of "issue linkages and policy options."

There is a good bibliography and topical index, as well as a glossary of "acronyms" and an index of "figures and tables." The latter provide helpful supplements to easy comprehension of the text. The book's only structural flaw is the relegation of footnotes to the rear, a device that reduces the utility of many serious professional books.

The book's appeal derives not only from Rochlin's verbal skill, but also from his ability to convey complex scientific ideas with minimal resort to mathematical syntax. In its restricted domain, the book bridges the "two cultures" (scientific and humanist) that C. P. Snow and others have expounded.

Rochlin illuminates the diverse obstacles entailed in negotiating arrangements for safer disposal of spent fuel (to which I would add obsolete and worn-out reactors). He has achieved what relatively few physicists have even attempted: an understanding that "any international agreement can be successfully negotiated" only if the "institutional and political demands it makes of member states are commensurate with the benefits *they perceive to occur*" (p. 2, emphasis added). This implicit inclusion of the political variables increases Rochlin's contribution to the congeries of proposals for controlling the nuclear-fission monster.

The book is a tour de force as it stands, but I suggest that it would be an even better book with a little more attention to a few additional considerations. Every proposal to make the nuclear fuel cycle safer includes some kind of benefit-cost analysis. Most attempts accept the premise that such calculations are expressible in monetary units. Yet this premise has limited tenability in the context of hazards, some of which are inherently non-quantifiable.

Another consideration is the depth of public concern regarding nuclear power development. Such concern may or may not exaggerate the hazards involved, but it has raised its head, nevertheless, at every stage from engineering design to ultimate disposition of residues.

One may doubt that this industry would ever have reached its present magnitude without huge subsidies of tax dollars. Only government commands the power to extract such sums from the economy. Only government commands the police power to enforce restrictive regulations. Only government has the authority to determine priorities among competing claims. This point, in particular, would benefit from more consideration. Numerous analysts have emphasized the fragility of high-technology societies. The higher the level of human density, technological capacity, and economic productivity, the larger become the costs of domestic and external security and many other imperatives. The recurring energy crisis is an exhibit. Another is the inflated military budget.

What if ever-increasing military and other claims on disposable public funds should continue indefinitely? What if the costs of safer nuclear power operations follow the same upward trend? What would be the likely impact on American participation in any program of agreements to abate the hazards of worldwide nuclear residues? What if doubts are confirmed that the energy required for building and operating nuclear power utilities produces little net increase in total available energy? What if private industries exhibit still more caution toward investing heavily in new nuclear fission utilities? These are some of the additional concerns of students of domestic and international statecraft.

The American electorate has been gulled by hubristic and deceptive propaganda. For example, nuclear-radiation hazards are compared superficially to deaths and injuries that affect only living people and their immediate offspring. Accidental massive escape of radioactive poisons into air, water and land could inflict damage that might persist from a few years to farther into the future than the centuries that have elapsed since the building of the pyramids.

Surely, these and other considerations are not news to Rochlin. I infer so, in part, because of his sophisticated rejection of grand designs to resolve all the problems at once. Instead, he opts for arrangements limited territorially and in political composition and scale, that in a longer future, *if given sufficient reprieve,* may incrementally evolve into an adequately comprehensive protective system.

HAROLD SPROUT

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The Limits to Power: Soviet Policy in the Middle East. Edited by Yaacov Ro'i. (New York: St. Martin's Press, 1979. Pp. 376. $22.50.)

This collection of 12 essays written, with one exception, by Israeli scholars contributes sophisticated, informative analyses of Soviet policy toward various aspects of the Arab-Israeli problem since the October War. The diplomatic, military, and economic dimensions of Moscow's policy and interaction with the Arab East are astutely examined within the framework of the ongoing Soviet-American quest for stability at the nuclear level and rivalry for influence in the Middle East. The essays are well researched, with extensive use of Russian and Arab materials, skillfully argued, and clearly written. In a word, the volume merits a valued place on the reference shelf of anyone interested in the subject.

The first two essays set the USSR's policy toward the Arab-Israeli conflict in a global context: Galia Golan focuses on the Soviet-U.S. interaction, the changing nature of Soviet aims, detente, and the post-October War ups and downs in U.S.-Soviet and Israeli-Syrian-Egyptian relations; Amnon Sella analyzes the military dimension of Soviet strategy, arms transfers, and involvement. Gus Ofer postulates that the USSR's growing military sales and aid in the Middle East "may reflect the development of a new Soviet economic interest rather than an intensifying of the existing strategic or political one" and concludes that Soviet commitments to the area are likely to increase in the foreseeable future. Dina Spechler and Martin Spechler provide an excellent statement on the USSR's interest in Middle East oil. They examine the policy toward the 1973 Arab oil embargo, nationalization of Western holdings, Arab price increases, and probable future needs, concluding that the Arabs' unsheathing of their oil weapon "has not been of much help to the Soviets in their political competition with the West," though there have been some short-term economic benefits.

Oded Eran imaginatively undertakes to examine how Soviet analysts see the setbacks to Soviet influence. Relying on the writings in key publications, he proceeds on the assumption that "in order to understand the Soviet theory of influence one has to look for the Soviet conceptual equivalents," which he decides are "solidarity" or "union" between the world socialist system and the main targets of Soviet attention in the Middle East. He observes that in the post-1973 period the writings have frequently dwelt on disappointments with developments in the Third World. Theodore H. Friedgut assesses the domestic image of Soviet involvement in the Arab-Israeli conflict. Yaacov Ro'i contributes two fine pieces, one on the Soviet-Egyptian relationship, another on the Soviet attitude toward the existence of Israel. Additional articles treat the Soviet-Syrian and Soviet-Palestinian relations, the Soviet view of a Middle East settlement, and an overall assessment of Soviet achievements and dilemmas.

This strong effort is not without a few shortcomings in terms of neglected areas and conceptual approach. First, there is the absence of any consideration of Jordan, Lebanon, and the Sudan. Given the authors' focus on the Arab-Israeli sector of Soviet involvement in the Arab world, the neglect of these countries is noticeable. Second, from my perspective at least, all the contributions tend to take an overly benign view of what the Soviet Union is up to in the area, especially during the past decade. They deal with strategic context, but are too quick to stress Soviet setbacks and downgrade the important gains that Moscow has made in encouraging the policies of regimes whose behavior is antithetical to Western interests. True, Soviet influence has suffered, but so has the West's, despite the new prominence of American policy since 1973. Finally, because most of the papers were completed by March 1977, the result is a premature attribution of diminished Soviet importance in the Arab world. The dramatic events of the 1977 to 1979 period require modification of the picture of the passing of the USSR from the Middle East scene. No doubt some of the conclusions would have been different had the cutoff date been March 1979. If Moscow is cautious, it is not because of concern about the United States, but because its Arab clients are intent on organizing, with Soviet backing, their own responses to U.S. initiatives. Moscow is following in the wake of the Arab rejectionists, providing the weapons needed to prevent a settlement of the Arab-Israeli conflict and make possible a military option for the future.

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The Nordic Council and Scandinavian Integration. By Erik Solem. (New York: Praeger Publishers, 1977. Pp. xi + 197. $19.95.)

Managing Transnationalism in Northern Europe. By Bengt Sundelius. (Boulder, Colo.: Westview, 1979. Pp. xii + 127. $14.50.)

Nordisk Raad 1952–1978. By Frantz Wendt. (Stockholm: Norstedt, 1979. Pp. 370. D.kr. 240; $60.00.)

Erik Solem has written the third English-language book on the Nordic Council. The first, by Frantz Wendt, entitled *The Nordic Council and Co-operation in Scandinavia* (Copenhagen: Munksgaard), was published in 1959, six years after the Council—an interparliamentary and intergovernmental consultative assembly—commenced. Wendt, for many years secretary-general of the Danish delegation to the Nordic Council, is the Grand Old Man of Scandinavianism. My own book, *The Nordic Council: A Study of Scandinavian Regionalism* (University of Washington) came out in 1967. The progression of titles from "co-operation" to "regionalism" to "integration" is misleading because "integration" is used interchangeably with "cooperation and coordination" in Solem's book (see particularly p. 29). Solem follows the basic organization used by Wendt in 1959, focusing on cooperation in different areas such as the law, communications, social policy, and economics, following the introductory chapters on the origins, structure and functions of the Nordic Council. He draws heavily on Wendt's book and on my book, even beyond the frequent footnote citations, and upon secondary sources in the Scandinavian languages. Basically, this is a scissors-and-paste book. As such, it could have been justified by the need to update the information available in English, particularly the striking growth in institutions and personnel surrounding the council. Unfortunately, Solem devotes far too much space to rehashing and paraphrasing what Wendt and I had already covered and too little space to subsequent developments.

In the foreword to Solem's book, Wendt attributes to Solem "a solid grasp of the various theories on regional integration" (p. v). Regrettably, this is not the case. As Bengt Sundelius points out in an incisive review of Solem's book in *Cooperation and Conflict* (1977, No. 4, p. 265), "The main weakness with the theoretical portion of the book is that the author has concentrated his analysis of integration theory on works published prior to 1970." Sundelius is referring to extra-Scandinavian authors, and he lists the more recent works which might have informed the Solem book. I would add that Solem has also ignored a Nordic scholar whose contribution predates 1970. Nils Andrén wrote a seminal article on "Nordic Integration" in the 1967 volume of *Cooperation and Conflict* (No. 1, pp. 1–25). Solem cites this article in this bibliography and uses Andrén's notion of "cobweb integration," but does not make direct reference to Andrén in the text nor accept Andrén's invitation to future scholars to carry out an intensive study of cooperation in some single area. Only when the minutiae of inter-Nordic collaboration on a particular topic have been collected and analyzed will we be able to measure the accuracy of Frantz Wendt's assertion in the foreword that "The Nordic Council is . . . the central driving force in present-day Nordic cooperation" (p. vi).

There is some original material in Eric Solem's book. He has queried parliamentarians, civil servants, and cabinet members on the "organization, methods and aims of the Nordic Council." The results of these surveys are included as appendices, and they are cursorily analyzed in chapter 12. One interesting item is the substantial concurrence in the proposition that the Nordic Council ought to "concentrate on fewer and more important matters." Thirteen out of 18 government members thought so, as did 18 out of 28 parliamentary members and 15 out of 33 officials (with 9 opposed and 9 uncertain) (pp. 174–76). Such a drastic reform would reduce the oft-criticized paperwork of the council—the three-volume official report on the 1978 session runs more than 2500 pages—and remove it as another advisory layer in the already exhaustive process of consultation which characterizes policy making in the Scandinavian countries. (Ironically, this would make the sessions of the Nordic Council more like those of its predecessor, the Nordic Inter-Parliamentary Union.)

Bengt Sundelius has written a more interesting and thoughtful book. *Managing Transnationalism in North Europe* grows out of his 1976 doctoral dissertation, "Nordic Cooperation: A Dynamic Integration Process," and expands his article on "Trans-governmental Interaction in the Nordic Region" in the 1977 *Cooperation and Conflict* (No. 2). Sundelius makes inter-Nordic relations part of the politics of incumbency: in order to remain in power, national political elites must cope with unavoidable interdependence (p. 2). This is accomplished through collective management. In an article entitled "Nordic Cooperation and High Politics" in the 1974 *International Organization,* Nils Ørvik suggests that scholars choose to study international organizations, and the Nordic Council in particular, because of their accessibility (p. 68). Sundelius starts with the council, but he recognizes "the importance of informal interactions in the region and the rather low ratio of formal meetings to actual policy results" (p. 61) and goes beyond analysis of the organs surrounding the Nordic Council. Based as they are upon participant-observation, Sundelius' characterizations are sound, and they add a commendable emphasis on the newer (1971) Nordic Council of Ministers. Again, however, the need for intensive case studies is evident.

Sundelius attempts to relate the process of management to policy outcomes. In the final analysis, however, it seems that the main outcome is the process itself "of consultation, deliberation, and decision . . . even at times of great failures or serious challenges" (p. 114). That "the Nordic cooperation process is basically indecisive, slow, and primarily unsuccessful" (p. 62) may stem from the fact that "the motive behind collective management is a desire to protect the societies and regimes from drastic change" (p. 114). In maintaining the status quo (which, in Scandinavia, is a defensible goal), the national bureaucracies are normally the forces of inertia ("vetoers," Sundelius calls them on p. 108) while the interregional (mainly Nordic Council) officials are initiators and mediators (p. 107).

Frantz Wendt exemplifies the spirit of pan-Scandinavianism. In his poem "Danmark, trofast" (Denmark, faithful), the satirical Danish author Klaus Rifbjerg captures this embodiment in the following fragment which I have translated. (Hindsgavl is the Danish retreat used for Nordic meetings, and the five swans traditionally symbolize Nordic unity.)

what about a peek at
Hindsgavl, the castle
you remember
up on the hill
the flag pole
it is always the end of May
and there stands Frantz Wendt
embracing five swans. . . .

Wendt's new book *Nordisk Raad 1952–1978* (The Nordic Council 1952–1978, subtitled "Structure, Work, Results") is not a revision of his 1959 book—although it covers the same categories—but is essentially an encyclopedia of the recommendations adopted by the council over a 25-year period and the action or inaction which has ensued. This is a painstaking and comprehensive volume, a protracted labor of love which reflects Wendt's profession as an historian. The pages are oversized and double-columned, which helps to explain the high price. (Wendt's 1959 book, which I noted in the 1960 volume of the *American Political Science Review*, cost only D.kr. 14, or about $2 by then-prevailing rates of exchange.) Both books were commissioned by the Nordic Council, and the one presently under review may be considered as an official publication, which adds further to its authoritativeness. It is anticipated that it will be translated into Finnish and that a shortened version will come out in English. Most of the items in the extensive bibliography refer to Scandinavian-language sources, and these could be eliminated—many are already included in *Books, Articles and Reports Concerning Nordic Cooperation* (Stockholm: Nordist Utredningsserie 1975:3)—but otherwise it will be difficult to compress further this tightly packed digest without simply eliminating entire sections from the present table of contents, whose 45 chapters are found in the following sections: historical background, institutions and organizations, economic cooperation, traffic problems, social and environmental problems, regional problems, uniform legislation, cultural matters, and relations with the rest of the world.

Bengt Sundelius' article (co-authored with Claes Wiklund, Secretary to the Nordic Council's Social and Environmental Committee) in the *Journal of Common Market Studies* (September 1979) is entitled "The Nordic Community: The Ugly Duckling of Regional Cooperation," and ends with the hope that the study of Nordic regional collaboration "perhaps . . . can at last find a place among the other Swans in the mainstream of international relations research" (p. 17). Although he does it cautiously here and in his book, Sundelius joins Wendt and Solem in the widely held belief that (p. 21): "Naturally, the creation of formal structures has . . . enhanced these [intergovernmental] interactions and increased the possibilities that policy results are achieved through the continuous [informal] contacts." I am not so sure and question the proportionality of the results. The Nordic Council and its directly associated organs appear to have about 80 employees, a more than tenfold increase over the past decade. More than 150 "experts" attended the 1978 annual session of the Nordic Council in Oslo, outnumbering the 45 Cabinet members and 78 Members of Parliament in attendance. Sundelius and Wiklund play down the size of this group of 400 individuals whom they estimate to comprise the total Nordic bureaucracy, by comparing their number to 15,000 employees in the European Community (p. 10).

I would argue that this quantum jump in Nordic personnel is a kind of *trøstespisning*, a Danish term for compulsive eating undertaken as a consolation for frustration. Every time a major proposal for military or economic integration falls through in Scandinavia, the Nordicists are compensated by the creation of more sinecures. A Hans Christian Andersen fable other than "The Ugly Duckling" suggests an equally plausible interpretation of this admittedly modest empire building. Imagine the Nordic "Emperor" parading in all his splendid raiment—the Nordic Council itself and its five national secretiats and five committees, fashioned with "a Nordic Council of Ministers, 15 Permanent Committees of Senior Officials, three Nordic secretariats, one fund for technological and industrial development, one Nordic investment bank, plus close to one hundred smaller institutions and committees responsible for limited policy sectors" (Sundelius and Wiklund, p. 7). A small child turns to his mother and says, "There is only a spiderweb under that fine clothing!" To repeat the theme of this review, empirical studies are needed to provide a measure of the substance which underlies the proliferating institutions of Nordic collaboration. In another article written with Claes Wiklund, "Nordic Cooperation in the Seventies: Trends and Patterns" (*Scandinavian Political Studies*, No. 2, 1979), Sundelius deplores "the lack of detailed empirical research on Nordic relations" (p. 113) and presents a program for further study. The conclusion of this program should help to tell us whether Frantz Wendt is embracing swans or goosing ducks.

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Studying Boundary Conflicts: A Theoretical Framework. By Sven Tägil, Kristian Gerner, Göran Henrikson, Rune Johansson, Ingmar Oldberg and Kim Salomon. (Lund, Sweden: Esselte Studium, 1977. Pp. 215. Price not listed.)

At first glance a study of boundary conflicts might portend a rather dry, hyperfactual approach to the subject. Happily, such expectations are not borne out in this book, for its treatment of boundary conflicts is both analytical and abstract. This volume is the first in a series of studies of boundary conflicts in Europe during the period between the First and Second World Wars. As the first book in this series, its primary orientation is toward epistemology and theory.

The authors define the goals of their project as expanding "knowledge of the problems of boundary conflicts and offering explanations for the courses of action pursued by individuals and groups in such conflicts" (p. 12). As a research site for pursuing these goals, the authors choose five boundary conflicts taking place in the critical pre-Second World War era. The reasons for choosing this particular area and time have to do not only with the availability of data, but more importantly with the prominence of boundaries in European politics during this period. Following the end of the First World War many European boundaries were redrawn with the goal of making nations as homogeneous as possible. As the authors note, the ethnically mixed character of populations in several areas made it impossible to achieve this goal. Subsequently, tensions did arise in these areas, tensions which were subject to manipulation and escalation by local and national political leaders. Against this background the authors are intent upon understanding the nature of these local conflicts and also of evaluating the extent to which they contributed to the occurrence of the Second World War.

To direct their investigation, the authors establish a theoretical framework, the most important elements of which are their epistemological assumptions, conception of conflict and the factors they hypothesize will explain the emergence and developments of the conflicts. The epistemological position adopted by the authors is that human beings act in accordance with perceived reality. Conflict emerges when actors perceive that their goals are incompatible with other actors with whom they must interact. However, in explaining the main variables in the theoretical framework the authors clearly recognize that goal-directed behavior, even though affected by perceptions, must also be shaped and constrained by the administrative, socioeconomic and cultural systems in which the actors are located.

While it is appropriate to begin a research report—particularly one which will cover several volumes—with an explanation of the project's theoretical position, at this point it would also have been helpful for the authors to have been more generous with a substantive account of the various boundary conflicts so that the theoretical arguments could be tied more systematically to the data. There is also another, larger aspect of the research which needs to be addressed. Throughout the world, and across time, how frequently do boundary disputes occur? How frequently do they result in violence? How do the cases considered in the present study relate to other boundary conflicts? Without a consideration of questions such as these, the case studies will ultimately stand in isolation, and one may wonder then if five isolated case studies are really worth the impressive theoretical structure contained in this book.

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Poverty: Wealth of Mankind. By Albert Tévoédjrè. (New York and London: Pergamon Press, 1979. Pp. iv + 182. $22.00, cloth; $11.00, paper.)

One hundred years ago, Proudhon proposed resolving the rich-poor dichotomy through removal of the poverty component of the relationship. Today the reverse surgical operation is being advocated by the Deputy Director-General of the ILO and Director of the International Institute of Social Studies. Albert Tévoédjrè, a citizen of Benin who is also a professor of political science at the Sorbonne, argues that the solution to the major issues of the end of this century will depend on a massive return to socioeconomic and political systems in which individuals' poverty provides the basis for collective wealth and well-being, to be achieved through the social control of needs.

To support his sanctified notion of poverty as the only route to mankind's salvation, the author quotes liberally from left and right, calling on such diverse authorities as the Bible, Marx, Bossuet, and Enrico Berlinguer. While this ease of quotation reveals the extent of the author's scholarship, it is unfortunate that many of the references are cited outside of their socio-historical context.

The stage is thus set for a book which turns out to be rich in facts about the state of economic, social and political disarray in both the developed and the developing worlds, but also riven with theoretical and methodological inconsistencies and a degree of political naivete and utopianism.

More questionable, however, is the author's idealization of the Japanese development strategy and the Chinese and Tanzanian models as routes for the rest of the Third World to follow. Japan's success was due to its imitation and importation of Western technology, Deng's China is much more pragmatic than Mao's, and "self-reliant" Tanzania is weighed down by foreign indebtedness. For a political standpoint, the author does castigate the ruling elites of most Third World countries for their "folly of mimicry." Yet his proposed "internal solidarity contract" falls far short of suggesting any concrete guidance as to how the existing socio-political and economic relations are to be replaced by the power of the "lords of the morning dew."

At the international level, the author focuses not on the logic of the existing socioeconomic system, but rather the will for power and domination on the part of some greedy nations. That instinct represents a subjective, immoral propensity which the author wishes to curtail through a "solidarity contract" among nations. Herein lies the ultimate flaw in Tévoédjrè's reasoning. His approach to reshaping the worldwide unequal exchange emphasizes moral exhortation relating to the ongoing discussions about the "New International Economic Order" rather than thoroughgoing analysis of the underlying causes of unequal exchange. He thus absolves the very Third World elites he reprimanded earlier in the book. In so doing, he implicitly vitiates the effectiveness of the NEIO to the extent that decisions about world market prices and therefore about "the lot of the producers of the developing countries" are made by the stock markets in the very heart of the capitalist system. Contrary to what the author says on p. 164, these institutions are anything but "unidentifiable centers of decision." He does speak of the need for "certain strongholds sheltering certain secret and privileged agencies" which affect the lives of millions to be "brought out into the full light of day, not only to satisfy the curiosity of the mind but to enable us to take the necessary action for our deliverance." The author goes so far as to urge, as a component part of the "necessary action" to be taken, that the producers themselves organize to overcome the machinery of the stock markets. However, the author laments that "unfortunately there is at present no such prospect" (p. 165). As a stop-gap measure, Tévoédjrè proposes what he calls a "research hypothesis" which is no less than a "ten-year moratorium on new transfers of technology to the developing countries, and on the export from those countries of non-processed raw materials" (p. 168). Unfortunately, however, since the book fails to act and to face the relevant "research question," i.e., how to organize the "producers" to dismantle the power of the "stock markets," his "research hypothesis" must be considered of limited usefulness.

In *Poverty: Wealth of Mankind* we have an eloquent *cri de coeur.* Its catchy title, its author's prestige and courage, and the relevance of its subject all help sustain our interest. However, inasmuch as the book ultimately fails to provide a focused framework for analyzing the phenomenon of world underdevelopment and its causes, it falls short of our expectations.

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View from the UN: The Memoirs of U Thant. By U Thant. (New York: Doubleday, 1978. Pp. xix + 508. $10.00.)

Dag Hammarskjöld's tragic death in a plane crash at Ndola in Zambia denied us any memoirs that he might have written covering his stewardship as secretary-general of the United Nations. His successor, U Thant of Burma, lived long enough to write what might be called selected recollections, which, though a valuable historical record by an important participant, is far from a detailed account of Thant's experiences as head of the world organization. One suspects that Hammarskjöld's version of his tenure would have been not only more literate and less repetitive, but also probably more sophisticated and incisive about the worldwide political forces that the United Nations must often contend with. But then again, Hammarskjöld was born into a political family (his father was for a time Sweden's prime minister during the first World War), while Thant was born a British subject in a part of the British Empire. And that's the rub.

Thant's life experience burdens the work and is reflected in the understandable resentment of having once carried the "white man's burden." Indeed, Thant admits that his conception of the United Nations was viewed primarily from the vantage point of the Third World, and that he perceived his role as secretary-general with colonialism in mind. In fact, he admits that in the 1950s he "identified with the cause of small nations, poor nations, newly independent nations, and nations struggling for independence" (p. 36).

In addition, Thant was moved by what can be fairly described as a simplistic quasi-Marxist view that Western domination of colonial areas had been based on the metropolitan's atavistic commercial policies, as well as those of its nationals, leaving the colonial domains merely primary producers, and industrially undeveloped. The end result was Western-induced poverty that led to the need for post-independence economic development which had to be a principal concern of the United Nations.

This narrow focus was in turn buttressed by Thant's childlike belief that conflicts, whether between nations or individuals, generally arose "not out of different viewpoints in their civilizations, not from reasons of their traditions and history, but from uncivilized elements in their character" (p. 26). To tackle these "uncivilized elements" Thant believed it was his task to "build bridges between peoples, governments, and states" (p. 27). To this approach he commendably held that a quiet tack which avoided public confrontation might often hold some hope of success.

He then argues that he was more than an administrator and his political role was justified under Article 99 of the Charter. The important difference, of course, is that his "quiet approach" can never be squared with the "public approach" seemingly justified under Article 99. The latter throws him into a public political vortex and goes far to erode whatever influence he might have behind the scenes or whatever bridge-building he might attempt between the disputing parties. Each public intervention will incrementally erode his position, for states annoyed at his action are not likely to trust, confide, or cooperate with him at some future point. Once bitten, twice shy. His admission that unlike states he was at a disadvantage in that he lacked the first-hand information upon which governments base their policies does not appear to have raised any doubts in his mind about the wisdom of the public approach. He wanted to have it both ways, but once you enter the public arena you will soon be branded by one side or the other as partial, and his recollections describe not once but many times when this occurred.

This said, Thant's recollections cast doubts on his political acumen. Reading between the lines, while keeping in mind his life experiences, one could argue with some cogency that Thant had a disguised hostility to the West that could not have gone unrecognized in Washington, London, and Paris, and might in part explain his thwarted political initiatives in many of the episodes he discusses. His gratuitous comments about American leadership—excluding Adlai Stevenson at the United Nations—support this assertion, whether it is about John McCloy, President Johnson, or Dean Acheson, whom Thant considered as "one of the most overrated diplomats of his time" (p. 175).

Conversely, the Russians are generally described in a kinder light, whether it be the peasant-like Khrushchev who rekindled memoirs of Thant's own hunble origins, or Kosygin, who, Thant informs us without tongue in cheek, has a "quiet dignity and courteous manner" unmatched by any world statesman he had met (p. 415). Not so for United Nations Ambassador Nikolay T. Fedorenko, whom Thant disliked for reasons never spelled out. As one would expect, neither Castro nor Nasser is manhandled, though obviously both of them had jeopardized the world peace that Thant worked so strenuously to preserve.

The core events in these recollections are the Vietnam War, the Congo and Dominican crises, the Cuban missile crisis, the Six-Day War and Thant's withdrawal of the United Nations Emergency Force in the Sinai, the Russian invasion of Czechoslovakia, the Indo-Pakistani fighting and the birth of Bangla Desh. The episodes picked by Thant for discussion are geared less to show how he executed his office than to justify the actions he took and to reply to the criticisms leveled at him, especially for his withdrawal of the United Nations Emergency Force in the Sinai during the Six-Day War.

One of the book's great values is the 40 pages of documentation appended to the volume. This documentation some times reproduces and other times supplements that found in the edited volumes by Cordier and Harrelson dealing with the public papers of the secretaries-general and previously examined in this *Review*.

Memoirs and recollections like those of Trygve Lie and U Thant can only be considered the tip of an iceberg. They are limited by the perceptions of the secretaries-general and especially by the lack of sufficient supplementary documentation regarding those states who were forced to react to the initiatives the secretaries-general undertook.

JAMES BARROS

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Eurocommunism and Détente. Edited by Rudolf L. Tökés. (New York: New York University Press, 1978. Pp. x + 578. $24.50, cloth; $10.95, paper.)

Rudolf Tökés has made a major contribution to the public debate on Eurocommunism by presenting nine essays that cover almost everything that anyone ever wanted to know about Eurocommunism. The book brings together for the lay person and student material that was before scattered in a number of different places and languages, but it does not offer anything new for those already steeped in the Eurocommunist literature. The nine essays are divided between four that provide excellent in-depth historical accounts of the circumstances that have molded the communist parties of Italy, France, Spain, and Portugal. The other five essays are concerned with the international contexts and forces that affect these parties.

Given the wide array of experiences represented by the four major communist parties in Western Europe, one is brought to ask the question of what makes these parties "Eurocommunist" in comparison to other parties in Eastern Europe or around the world. The authors agree that three characteristics can be used to describe this new type of communist grouping. The first is the adoption of a domestic strategy that places emphasis on the gradual nature of reforms aimed at changing society's socioeconomic structure. As expressed by Robert Legvold, communist parties in the West can no longer be conceived as strictly revolutionary parties. They are concerned with "curbing rather than overthrowing dominant interests, exploiting rather than destroying bourgeois political institutions, enlarging rather than transforming the economic and social welfare benefits of the system, altering rather than replacing policy priorities" (p. 329). Such a view is translated in Spain into the PCE's *democracia politica y social* and the Italian party's attempt to socialize the economic rewards of the system rather than the means of production.

A second important element brought to the fore by Eurocommunism is the increased sense of autonomy from the Soviet Union and a rejection of the limiting features of past interpretations of "proletarian internationalism." Santiago Carrillo, Secretary General of the Spanish CP, has expressed this interpretation of the meaning of international communist solidarity in the following manner:

> There must be no guiding party or ruling center. No party has the right to impose its views on another or to interfere. Each party has its own forms and methods of action which are different from someone else's. These are general principles we regard as essential to arrive at a new unity—a unity of diversity of the Communist movement and of the anti-imperialist forces (p. 237).

In his chapter on the diplomacy of Eurocommunism, William Griffith presents convincing evidence to support the thesis that the foundations for the reorganization of the international communist movement were laid by Palmiro Togliatti in the 1950s and 1960s in his attempt to block Moscow's efforts to expel the Chinese from the movement. What gave these ideas special import for the Western European communist parties in the 1970s was the realization on the heels of the Czech invasion and Chilean events that the United States and the Soviet Union had struck a deal permitting continued American hegemony over the West and Soviet control over the East. The reasoning followed that detente and peaceful coexistence would not provide the conditions for a relaxation of tensions and reknitting of the socioeconomic fabric of a divided Europe. On the contrary, detente à la Breshnev and Sonnenfeldt doctrines meant a consolidation of the two blocs and increased competition at all levels.

The third characteristic of Eurocommunism is the growing emphasis on a European, or third line of development, that steers a course for European affairs between the Russian Scylla and the American Charybdis. John Campbell describes the foreign policy of Eurocommunism as a new variety of Titoism—that is, an attempt to create an independent bloc of national communist forces that is beholden to nobody and not tied to either bloc. The advocates of Eurocommunism feel that only in such a context will it be possible to reestablish, as Pierre Hasner points out in his penetrating essay, "a measure of unity and control to counteract the inequalities of uneven development and the anarchy of 'dispersed conflictuality' " (p. 60).

It is recognized by all of the authors that the development of Eurocommunism cannot but have an effect on Eastern Europe and that the Soviet Union would not accept this new brand of communism close to its borders. However, we could ask the same question of the United States. The numerous attempts by the Spanish and Italian communists to establish ties with the American government have been consistently rebuffed because we, like the Russians, seem to prefer governing parties in allied countries that are cast in our own image. But this point is completely missed by the authors. They assume that the burden of proof lies with the Eurocommunists and not with both sides of the hegemonic relationship. However, not everything can be done in one book, and what the authors set out to do they do well.

ROBERT LEONARDI

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The SALT Experience. By Thomas W. Wolfe. (Cambridge, Mass.: Ballinger Publishing Co., 1979. Pp. xvi + 405. $18.50.)

The Soviet-American Strategic Arms Limitations Talks (SALT) that extended for more than a decade resulted in the SALT I (1972) and SALT II (1979) agreements. The SALT negotiations began during a hopeful decade for arms control, one that saw the establishment of the U.S. Arms Control and Disarmament Agency in 1961 and the ratification of the Test Ban Treaty in 1963. Due perhaps in part to an oversold SALT I agreement, disappointment accompanied the drawn-out SALT II negotiations. The SALT II treaty was signed in June 1979 amid continuing concern over the Soviet arms buildup and increasing restiveness among national security specialists about the future of arms control (e.g., the observations of Leslie Gelb and Richard Burt in the Fall, 1979 issue of *Foreign Policy*).

The first decade of SALT negotiations is bracketed in print by two books: John Newhouse, *Cold Dawn* (Holt, 1973) and Strobe Talbott, *Endgame* (Harper, 1979); the former concluded with the SALT I agreement in 1972; the latter examined the 1977–79 period that culminated in the SALT II treaty. The book reviewed here examines the entire SALT process. Written by a respected specialist on Soviet strategy, the book consists of 263 pages of text, 93 pages of notes, an index and five appendices, including a useful glossary of acronyms and technical terms and a statement of the main features of the SALT II agreement as of January 1979. Although the last appendix is in obvious ways superseded by the treaty itself, the author's comments on the evolution of various provisions are helpful.

This book concentrates on three main areas. One concerns the American and Soviet institutions—civilian and military, from middle to highest levels—that have been involved in a decade of SALT policy making and negotiations. Here Wolfe has made a start in what could be called the comparative bureaucratic politics of arms control, an area whose further exploration might improve on "action-reaction" and "technological-imperative" explanations for arms competition.

A second area deals with the evolution of Soviet and American strategy and its relationship to SALT. One of the author's chief concerns is that the U.S. and Russia do not share a strategic culture: i.e., the USSR does not accept American assumptions about mutual assured destruction and Soviet strategy has not fundamentally changed during the SALT process. However, the Russian tendency to equate deterrence with warfighting capability "does not mean that they have been planning to start a nuclear war" (p. 249). Because of the significance of such assumptions in strategic theory, Wolfe might have addressed the arguments of a fellow sovietologist (and key participant in the SALT I negotiations), Raymond Garthoff, who maintains that articles in *Military Thought*, a confidential Soviet general-staff journal, indicate an acceptance of mutual deterrence by important Soviet military as well as the political elite (*International Security*, Summer, 1978).

The third area, which received more attention than the others, comprises the major issues in the search for a SALT II agreement from 1972 to 1978. Although for the 1972–1976 period the book does not provide the as yet untold "inside story" that would link *Cold Dawn* and *Endgame*, it carefully sifts and weighs the materials in the public media during those years.

Wolfe deals evenhandedly with a host of controversial issues (political, strategic, technological) and how they interconnect, frequently summarizing the views of various schools of thought. In his concluding reflections, where he admits to revealing his biases, his judgments remain balanced and prudent.

The network of techno- and politico-strategic relationships in the SALT process is illustrated by the anti-ballistic missile (ABM) and multiple independently targeted re-entry vehicle (MIRV) issues. Wolfe points to some of the ironies in their evolutions: the American MIRV that was conceived in the early 1960s as a countermeasure to a potential Soviet ABM later permitted unilateral American acceptance of a ceiling on its delivery vehicles, which in turn prepared the way for SALT negotiations. At the same time, the USSR was alarmed over the rapid advancement of the American ABM as well as MIRV. In 1970, as the U.S. deployed MIRV and Soviet planners were deciding on five-year weapons programs (including MIRV), neither side seemed seriously interested in pursuing a MIRV ban. The irony today is that the prospective vulnerability of American land-based missiles to Soviet MIRVs has stimulated interest again in the ABM: what some called the major failure of SALT I (no MIRV ban) is now undermining what was hailed as the primary success of SALT I, the ABM ban.

Speculating briefly about the impact of the SALT process (neither "millenium" nor "fruitless endeavor"), Wolfe expresses modest hopes for success in the decade ahead for achieving strategic stability as the superpowers "persevere (in the SALT process) to enhance their own security" (p. 263). He is aware of the tough problems that remain as each succeeding SALT tries to adjust the asymmetries left by the previous agreement: e.g., reductions and modernization constraints; "gray area" systems; ICBM vulnerability; strategic defense, including mobile missiles and "possibly reopening of the ABM treaty"; and military use of space (p. 259).

Debate over these issues and the relationship of arms control to national security in the 1980s will be enhanced by this cogent analysis of the "SALT experience" since the 1960s.

EUGENE J. ROSI

Dickinson College

China's Role in World Affairs. By Michael B. Yahuda. (New York: St. Martin's Press, 1978. Pp. 298. $18.50.)

Michael Yahuda has produced a descriptive work, with some useful insights. The treatment is historical, moving through periods of China's foreign relations from 1949 to 1977, but there are some gaps in the coverage which could have been overcome with more use of historical method. Much of the interpretation at the end is built on a sympathetic and not very forward-looking assessment of the Three Worlds editorial in the *People's Daily* on November 1, 1977. This is presented as a Maoist document that has set the orientation of contemporary Chinese external policy quite decisively, although when it appeared the stage was clearly being set for a closer relationship with the U.S.A. than the editorial would seem to permit. Yahuda evidently wishes to convey an impression of continuity in Chinese external relations, and even in Peking's domestic affairs. There are no references to the second purge of Teng Hsiao-ing in 1976 or the elimination of Mao Tse-tung's close associates—the "Gang of Four"—later that year. A false impression of unity in the present leadership is given by the omission of references to the intra-elite differences that were apparent immediately after the second rehabilitation of Teng Hsiao-ping in mid-1977.

There is little discussion of the sources of Chinese foreign policy, or of Peking's decision processes. The work simply presents China as a unified rational revolutionary international actor which responds consistently to challenges from the advanced capitalist states and the USSR. The text contains a vast number of assertions which begin: "The Chinese hold. . . ," "the Chinese view. . . ," or "The Chinese reject. . . ." Yahuda is almost certainly not unaware of divisions within the Chinese leadership, but he evidently does not wish his readers to learn about them.

The work amounts to a rather simplistic review of what might be called the "outputs" of the Chinese foreign policy process. In this respect its most serious deficiencies are its superficial treatment of relationships with the USSR and the U.S.A., and its failure to examine the Chinese connection with Japan. The dynamics of Sino-Soviet and Sino-U.S. interaction are given very little attention, and there is no discussion of Peking's options and relative successes and failures in these two vital areas of external relations. The Sino-Japanese interaction, although it is a very significant example of successful dealings with the most vigorous political economy in the world, receives only passing mention. Chae-Jin Lee's excellent study of this subject is not mentioned in the select bibliography.

Perhaps the gravest fault in the work is its neglect of the elementary requirement for an account of the *attributes* of the Chinese regime. The absence of such an account is exasperating, especially because this regime has been the most unstable global actor on the international scene, and the current uncertainties about its external policy orientation, which are common topics of discussion in foreign affairs journals, are related to ongoing high-level struggles over questions of ideology and modernization.

For an example of what can be achieved by scholarship that is basically historical and descriptive, the reader should turn to Samuel S. Kim's volume, *China, the United Nations, and World Order* (Princeton, 1979). This is a much more valuable work which begins to answer some of the questions which international relations scholars can ask about Chinese foreign policy.

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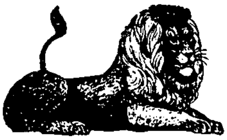

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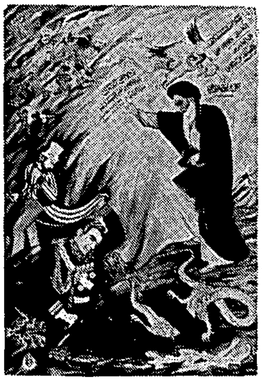

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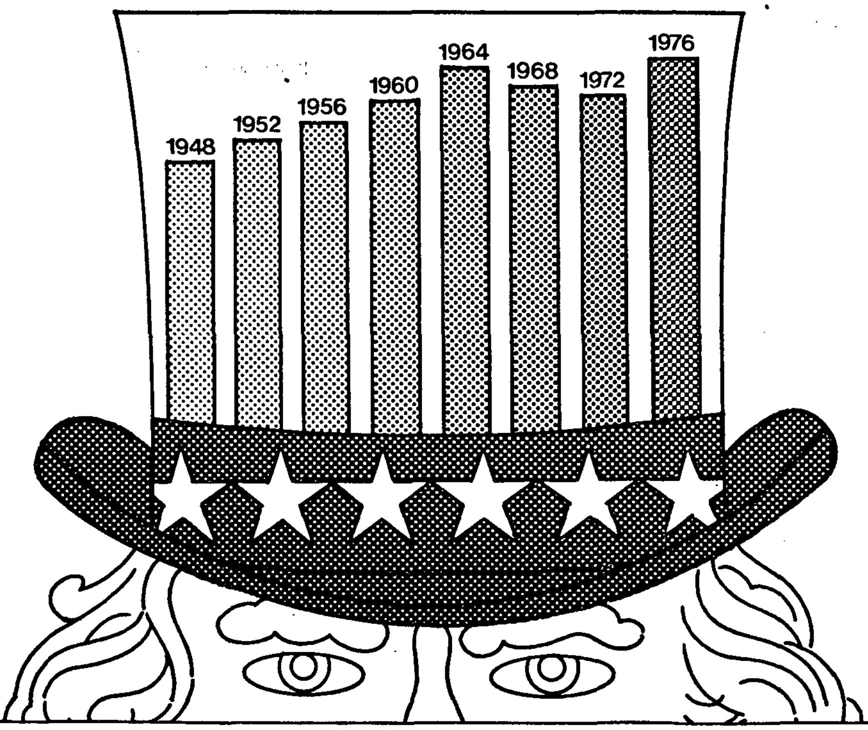

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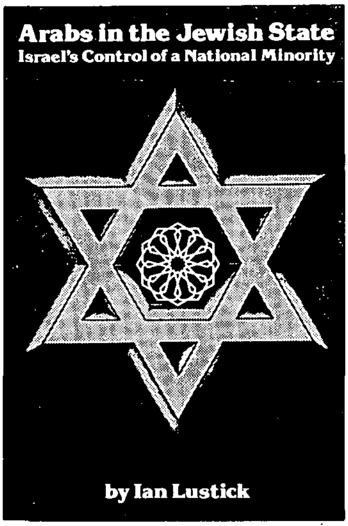
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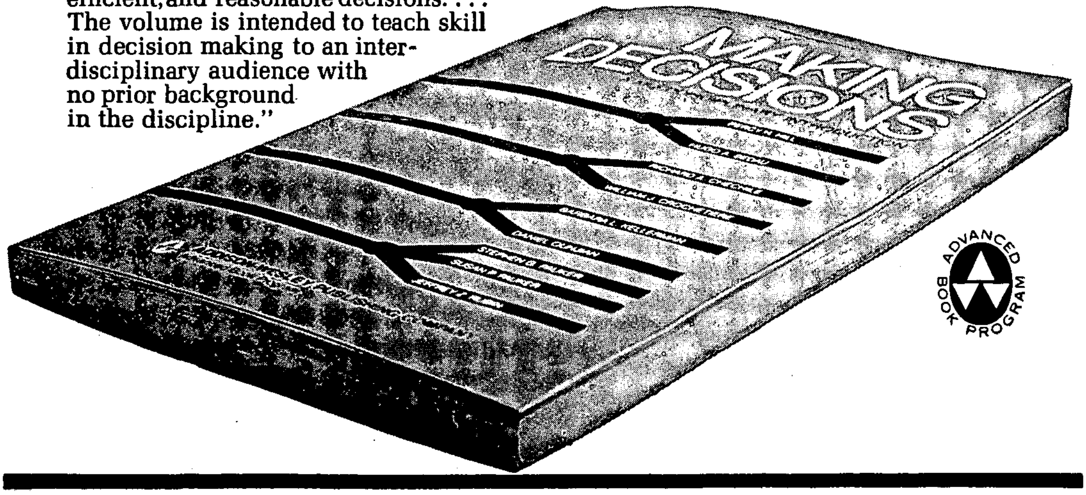

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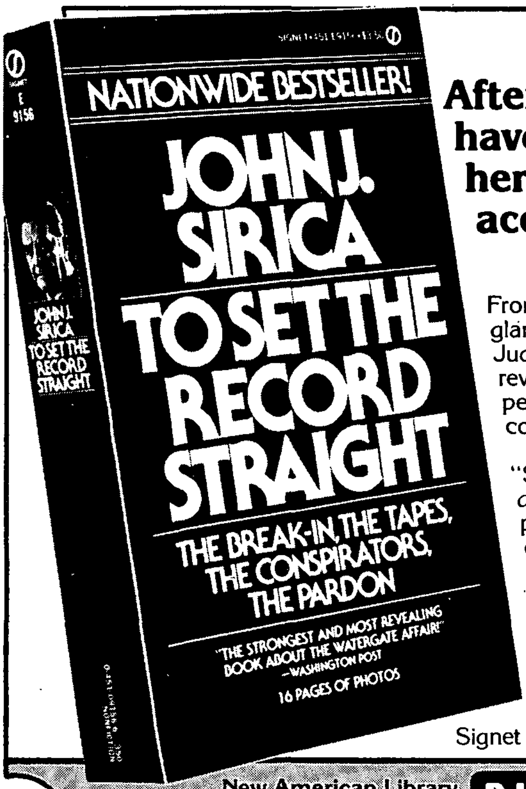
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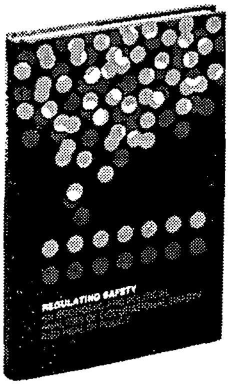

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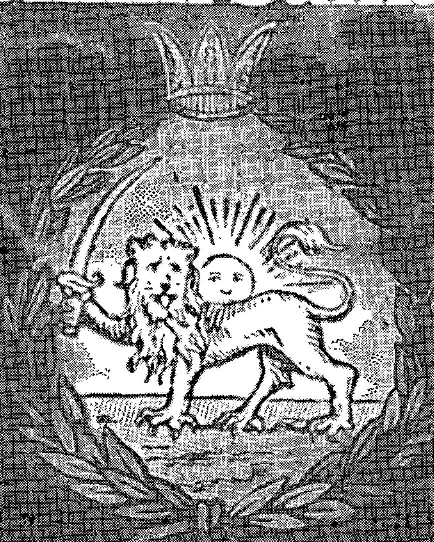

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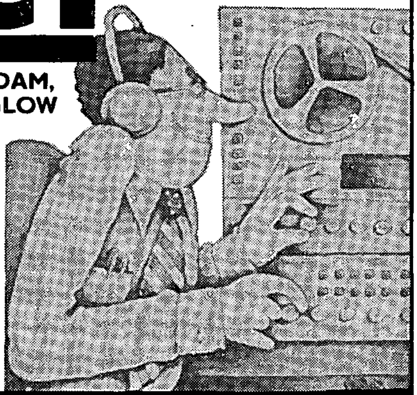

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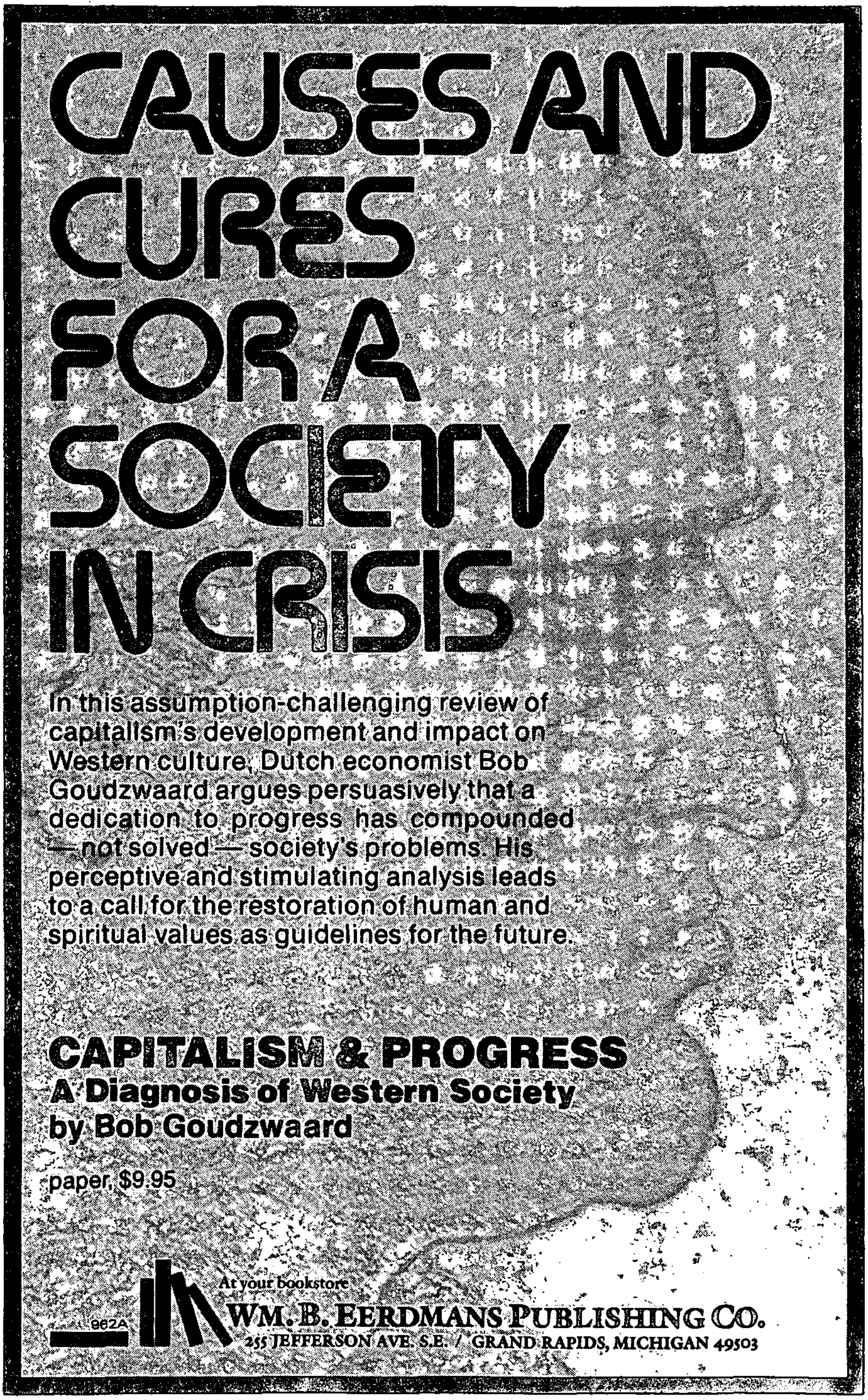

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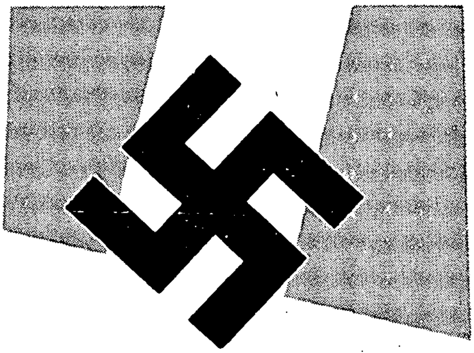

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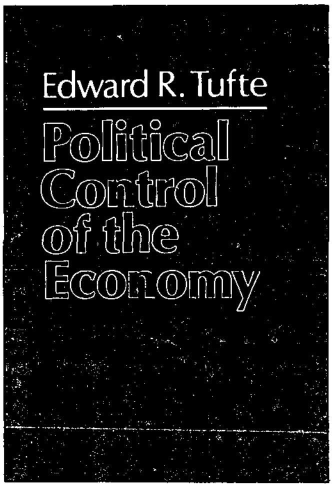

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